

Rs. 2.50  
\$ 0.50

# Theories of Motivation in Learning

by Richard C. Teevan

C. Birney

544  
TEE

AN INSIGHT BOOK BY VAN NOSTRAND

VAN NOSTRAND  
EAST-WEST PRESS

## THE EDITORS

RICHARD C. TEEVAN taught at the University of Michigan and at Smith College before assuming his present position as Associate Professor of Psychology at Bucknell University. A graduate of Wesleyan University, he received his Ph.D. from the University of Michigan. Professor Teevan is co-author of the *Guidebook* and *Teacher's Manual for Hilgard's Introduction to Psychology*; and co-editor, with Professor Birney, of four previous Insight Books.

ROBERT C. BIRNEY is Associate Professor of Psychology at Amherst College. Like his co-editor, he graduated from Wesleyan University and received his Ph.D. from the University of Michigan. Professor Birney collaborated with Professor Teevan in editing four previous Insight Books: *Instinct*, *Reinforcement*, *Color Vision*, and *Measuring Human Motivation*; he is also co-editor, with Robert F. Grose, of *Transfer of Learning*.

2102  
18.3.69

# THEORIES OF MOTIVATION IN LEARNING

An Enduring Problem in Psychology

SELECTED READINGS

*Edited by*

RICHARD C. TEEVAN

*Bucknell University*

AND

ROBERT C. BIRNEY

*Amherst College*

AN INSIGHT BOOK



(AN EAST-WEST EDITION)

D. VAN NOSTRAND COMPANY, INC.

TORONTO

PRINCETON, NEW JERSEY  
NEW YORK

LONDON

AFFILIATED EAST-WEST PRESS PVT. LTD.  
NEW DELHI

D. VAN NOSTRAND COMPANY, INC.  
120 Alexander St., Princeton, New Jersey  
(Principal Office)  
24 West 40 Street, New York 18, New York

D. VAN NOSTRAND COMPANY, LTD.  
358, Kensington High Street, London, W.14, England

D. VAN NOSTRAND COMPANY (Canada), LTD.  
25 Hollinger Road, Toronto 16, Canada

COPYRIGHT © 1964, BY  
D. VAN NOSTRAND COMPANY, INC.

Published simultaneously in Canada by  
D. VAN NOSTRAND COMPANY (Canada), LTD.

*No reproduction in any form of this book, in whole or in part (except for brief quotation in critical articles or reviews), may be made without written authorization from the publishers*

414  
EE  
AFFILIATED EAST-WEST PRESS PVT. LTD.

*East-West Student Edition-1968*

*Price in India: Rs. 2.50*

*Rest of Asia: \$ 0.50*

*Sales Territory: Asia*

*(Except Japan & the Phillipines)*

*Reprinted in India with the special permission of the original Publishers, D. Van Nostrand Company Inc. Princeton, New Jersey, U.S.A. and the copyright holders.*

*This book has been published with the assistance of the Joint Indian-American Standard Works Programme.*

Published by W.D. Ten Broeck for AFFILIATED EAST-WEST PRESS PRIVATE LTD., C 57 Defence Colony, New Delhi 3, India, and printed by S. M. Balsaver at USHA PRINTERS, National House, Tulloch Road, Bombay 1, India.

S.C.E.R.T., West Bengal

Date...18.3.69.....

Acc. No...2102.....

## Foreword

*By the Editors of the Series*

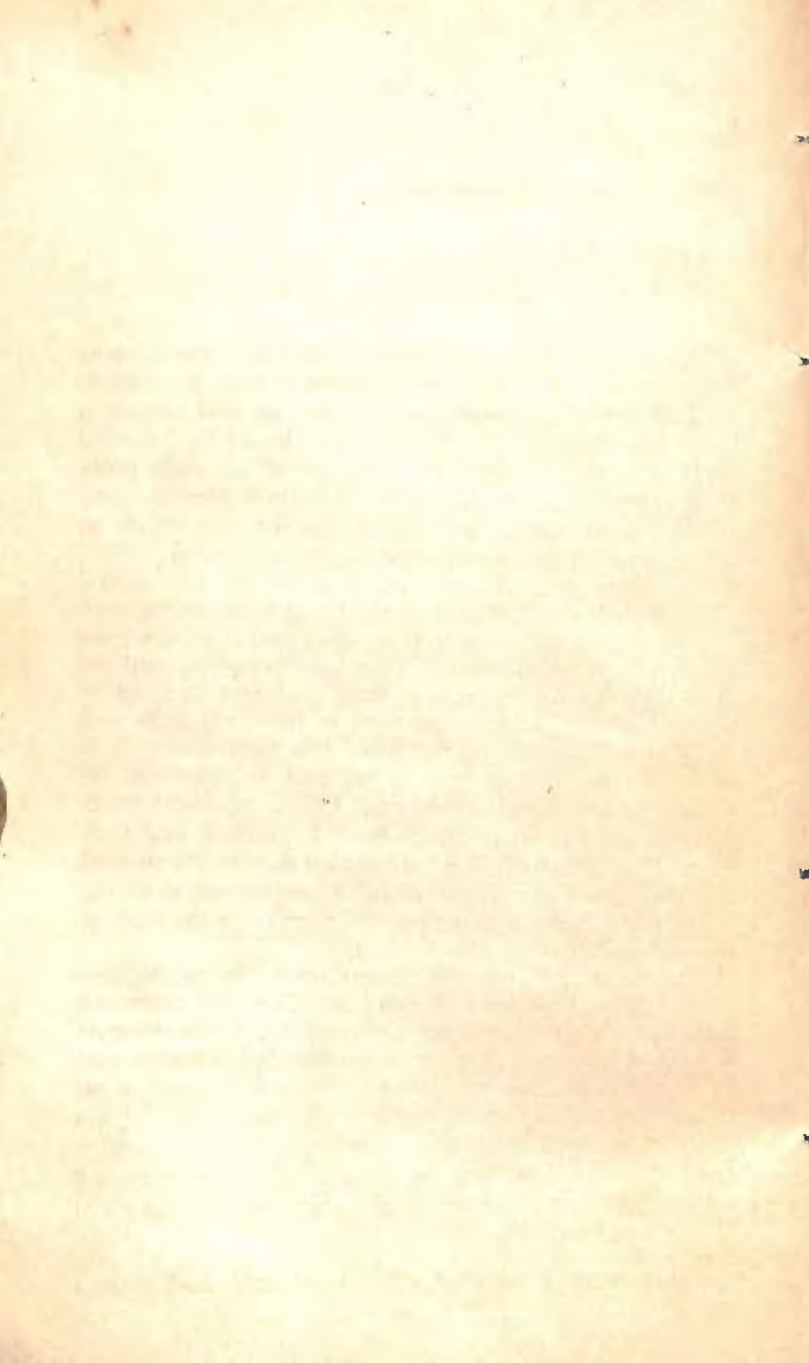
In the field of psychology we believe that the student ought to get the "feel" of experimentation by reading original source materials. In this way he can acquire a better understanding of the discipline by seeing scientific ideas grow and change. However, one of the main problems in teaching is the limited availability of these sources, which communicate most effectively the personality of the author and the excitement of ongoing research.

For these reasons we have decided to edit several books,\* each devoted to a particular problem in psychology. In every case we attempt to select problems that have been and are controversial—that have been and still are alive. We intend to present these problems as a set of selected original articles, arranged in historical order and in order of progress in the field. We believe that it is important for the student to see that theories and researches build on what has gone before; that one study leads to another, that theory leads to research and then to revision of theory. We believe that *telling* the student this does not make the same kind of impression as letting him see it happen in actuality. The idea is for the student to read and build ideas for himself.

*Suggestions for Use*—These readings books can be used by the student in either of two ways. They are organized so that, with the help of the instructor (or of the students if used in seminars), a topic can be covered at length and in depth. This would necessitate lectures or discussions on articles not covered in the series to fill in the gaps. On the other hand, each book taken alone will give a student a good idea of the problem being covered and its historical background as well as its present state and the direction it seems to be taking.

---

\* (Pub. note: a sub-series within the Insight Book Series.)



## Introduction

Most learning theorists have found it necessary to make some statements about motivation as a part of their learning theories. Up to this time there has been no single source to which one could send students interested in this aspect of the learning theories. In this volume we have attempted to remedy this lack by compiling what we consider to be the most important and most fruitful ways in which motivation has been used in learning. This book and its companion, *Theories of Motivation in Personality and Social Psychology*, reflect the manner in which various system builders have approached motivational questions.

Clark Hull's conception of motivation as primary drive states having a critical role in learning has generated lines of research which are still radiating. This is nowhere more evident than in the necessity felt by subsequent theorists to address themselves to the Hullian position. The other viewpoints contained in this collection all show this sense of departure from Hull. Miller and Dollard present the simplified construct system which has proved so useful in the analysis of human directional tendencies, and in so doing reveal a second component of learning theories having implications for motivation, i.e., the cue as having both arousing and directing effects.

Tolman's model of behavior places motivation at the center of the learning experience, emphasizing cognitive conditions whose organization literally dictates behavior having purposeful qualities. Thus the phenomena of motivation are absorbed in a model of cognitive structures and dynamics, thereby leading to a restatement of the interaction between motivation dynamics and the acquisition of new behaviors. Moving in exactly the opposite direction is Judson Brown's latest formula of the Drive construct. Brown's solution is to confine the Drive term to those initial arousal states resulting from tissue needs, and deal with all subsequent recurrences of these states in terms of the acquired systems for reducing them.

This places goal-directed behaviors squarely among the habit systems and interprets motivational phenomena as special forms of habit organizations.

Both Nissen and Woodworth raise questions which had gone out of style for some time. Can it be that primary drive states do more than simply arouse general activity? What evidence is there that drive states actually arouse rather well organized behavioral systems whose form and sequence are determined at the genetic level? Woodworth raises the issue of the motivational component in any major function such as perception, and Nissen suggests that if we say capacity is its own motivation we can unify such diverse systems as the psychomotor and biogenetic. The actual sequence of goal-directed behaviors may be learned, but both the arousal systems and the consummatory systems are innate and truly motivational in character.

Estes, on the other hand, demonstrates that sufficient evidence exists to support the view that it may be reasonable ". . . to account for as many relationships as possible in terms of the apparently indispensable concept of drive stimulus, leaving the notion of the general energizing factor to be brought in only if it really turns out to be necessary." The implications of this thrust become clear when he quickly asserts, ". . . I assume that drive stimuli are simply stimuli, with no special properties whatsoever." Estes then presents the evidence in support of this theory of the origins of motivational properties in behavior.

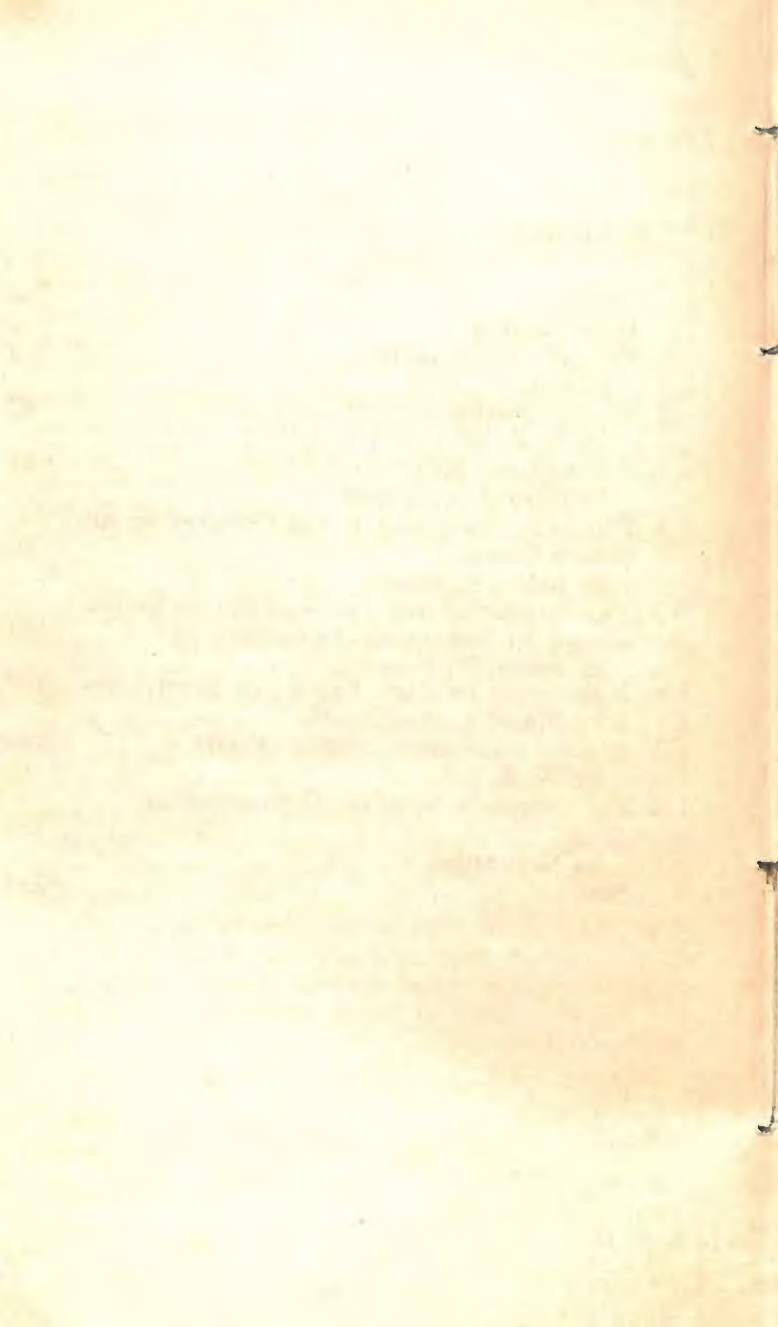
The last selection is Neal Miller's recent summing up of the diverse theories and researches dealing with the role of motivation in learning theory. Miller localizes the new controversies, states the contending points of view, and gives his personal sense of the new directions already emerging in the literature.

*Lewisburg, Pa.*  
*Amherst, Mass.*

R.C.T.  
R.C.B.

# Contents

FOREWORD	iii
INTRODUCTION	v
1. PRIMARY MOTIVATION	1
by Clark L. Hull	
2. FOUR FUNDAMENTALS OF LEARNING	42
by N. E. Miller and J. Dollard	
3. A COGNITION MOTIVATION MODEL	61
by Edward C. Tolman	
4. PROBLEMS PRESENTED BY THE CONCEPT OF AC- QUIRED DRIVES	79
by Judson S. Brown	
5. THE NATURE OF THE DRIVE AS INNATE DETER- MINANT OF BEHAVIORAL ORGANIZATION	103
by Henry W. Nissen	
6. A BEHAVIOR PRIMACY THEORY OF MOTIVATION	137
by Robert S. Woodworth	
7. STIMULUS-RESPONSE THEORY OF DRIVE	148
by W. K. Estes	
8. COMMENTS ON SELECTED CONTROVERSIAL ISSUES	180
by Neal Miller	
INDEX	214



## Primary Motivation

CLARK L. HULL

*This excerpt begins with Hull's 1943 statement of the "law of primary reinforcement" and then goes on to spell out his theory of motivation in greater detail. The evolutionary reasoning behind his theory can be found on pages 17 and 18 of the work from which this excerpt is taken.*

*Although Hull changed his statements concerning primary reinforcement between 1943 and 1952, we have used the 1943 statement as the beginning article in this book because it is this position which gave rise to Miller's position and also to Hull's later position. It is also true that Hull was never completely convinced and left the matter open. (See page 153 of A Behavior System, Yale University Press, 1952 and also Hilgard, E., Theories of Learning, Second Edition, Appleton-Century-Crofts, 1956.) In 1943 he took the position that primary reinforcement had to do with the diminution in a need while in 1952, following Miller, he changed to drive stimulus reduction as the central fact in reinforcement.*

### PRIMARY REINFORCEMENT

#### Summary

The infinitely varied and unpredictable situations of need in which the higher organisms find themselves make any form of ready-made receptor-effector connections inadequate for optimal probability of survival. This

---

Excerpted from Clark L. Hull, *Principles of Behavior*. New York: Appleton-Century-Crofts, Inc., 1943, with the permission of the publisher.

natural defect of inherited reaction tendencies, however varied, is remedied by learning. Learning turns out upon analysis to be either a case of the differential strengthening of one from a number of more or less distinct reactions evoked by a situation of need, or the formation of receptor-effector connections *de novo*; the first occurs typically in simple selective learning and the second, in conditioned-reflex learning. A mixed case is found in which new receptor-effector connections are set up at the same time that selective learning is taking place.

An inductive comparison of these superficially rather divergent forms of learning shows one common principle running through them all. This we shall call the *law of primary reinforcement*. It is as follows: *Whenever an effector activity occurs in temporal contiguity with the afferent impulse, or the perseverative trace of such an impulse, resulting from the impact of a stimulus energy upon a receptor, and this conjunction is closely associated in time with the diminution in the receptor discharge characteristic of a need, there will result an increment to the tendency for that stimulus on subsequent occasions to evoke that reaction.* From this principle it is possible to derive both the differential receptor-effector strengthening of simple selective learning and the acquisition of quite new receptor-effector connections, characteristic of conditioned-reflex learning as well as of certain forms of selective learning. Pavlov puts forward the alternative hypothesis that the critical element in the reinforcing state of affairs is the occurrence of the unconditioned stimulus. This formulation fits conditioned-reflex phenomena but breaks down when applied to selective learning situations, a fact which shows it to be an inadequate inductive generalization. Fortunately the inadequacy of this interpretational detail of Pavlov's work in no way detracts from the scientific value of the great mass of empirical findings produced by his laboratory.

### Notes

Is the Reinforcing State of Affairs in Learning Necessarily the "Effect" of the Act Being Reinforced?

It has already been suggested that the hypothesis as to the reinforcing state of affairs adopted in the present work is distinctly related to that of Thorndike's "law of effect." Thorndike seems to have coined this expression because the state of affairs which has been found empirically to be necessary in order to produce differential reinforcements, as in Demonstration Experiments A, B, and C,<sup>1</sup> under ordinary circumstances comes literally as the effect of the reaction which is reinforced. This cause-and-effect relationship is shown explicitly by means of the wavy-shafted arrows leading from the act reinforced, e.g.,  $R_4$  in Figures 13 and 14,<sup>1</sup> which patently causes the termination of the injurious action to the foot tissue and of the receptor impulses,  $s_D$ . A strictly parallel though slightly more complex causal relationship is seen in Figure 16,<sup>1</sup> where  $R_u$  terminates the current on the grid by interrupting the circuit and this, in turn, terminates the shock to the foot and at the same time brings to an end the receptor impulses ( $s_u$ ) arising from the current passing through the receptor organs buried in the tissue.

At first thought it might be supposed that since only consistent reinforcement will set up stable habits, and since the reaction ( $R$ ) when in a given situation yielding  $S$  to the receptors will be followed consistently by a reinforcing state of affairs only when there is a causal connection between the antecedent events and those which follow, the "law of effect" would be established on a firm *a priori* foundation. In point of fact, however, this rule breaks down in the Pavlovian conditioned-reflex experiment where the salivary reaction of the dog can by no stretch of the imagination be regarded as the cause of the receipt of food which reduces the hunger and is commonly considered the reinforcing agent in this experiment. This paradox is explained by observing that some *common* cause, the food, produces first the salivation and, later, the reduction in the need (hunger). Accordingly the reinforcing state of affairs is temporally related to the reaction involved in the reinforcement in strict accordance with the law-of-reinforcement formulation,

<sup>1</sup> [Pages preceding this excerpt.]

but not as the effect of the reaction being reinforced. A second presumptive exception to Thorndike's formulation of the nature of the reinforcement process is the conditioned kneejerk (10). The termination of the receptor discharge from the slightly injurious blow on the patellar tendon occurs because of the brief duration of the impact of the hammer, rather than because of the occurrence of the kneejerk.

Despite these minor exceptions, Thorndike's inductive generalization, as represented by the expression, "law of effect," is based upon a very penetrating bit of scientific insight into the dynamics of adaptive situations in general. Nevertheless the exception is probably genuine and it has seemed best to employ in the present work the slightly more appropriate though less colorful expression, *law of reinforcement*.

*What Is the Critical Factor in Primary Reinforcement?* In Figures 13, 14, and 16 it will have been observed that reductions in (1) the need and (2) the receptor response to the need *both* follow as consequences of the act involved in the reinforcement process, directly in Figures 13 and 14 and indirectly in Figure 16. These considerations raise the question as to which of the two is to be regarded as the critical reinforcing agent; this can be determined only when some radical experiment is performed in which one of the two is eliminated and the other remains active. The writer is aware of no critical evidence of this kind. Until such becomes available the issue must remain uncertain. Meanwhile, in the interest of definiteness, the alternative of reduction in drive-receptor response is chosen for use in the present work as the more probable of the two. Should critical evidence later prove this choice to be in error, a correction can be made. In the present stage of our ignorance regarding behavior dynamics, an error in either direction would not seem to have such far-reaching systematic implications as to render correction unduly difficult.

*The Effectiveness of Reinforcement and the Intensity of the Need Involved in the Reinforcement.* A recent study by Finan (4) tends to support the view that reduction of need is a critical factor in the primary reinforcement process. This investigator trained groups of

small bar in a Skinner-Ellson apparatus. Each group of animals received the same number of reinforcements but in a different condition of food privation. Two days later, after the food need had been equalized, all groups were extinguished. The median number of non-reinforced reactions required to produce a constant degree of experimental extinction were as follows:

Hours of food privation during reinforcement	1	12	24	48
Median number reactions to produce extinction	25	57.5	40	41

While the above extinction values indicate that the relationship is not a simple increasing function of the number of hours' food privation at the time of reinforcement, they do show that for some hours after satiation there is a progressive increase in the effectiveness of reinforcement; thus the two phenomena are shown definitely to be connected.

*The Onset, Versus the Termination, of Need-Receptor Impulse as the Critical Primary Reinforcing Factor.* The view has been put forward in the preceding pages that the termination of need-receptor impulse is the critical factor in the primary reinforcement process. Many students in this field, however, have held the view that reinforcement is critically associated with the onset of the need or drive, as represented by the physiological shock in Demonstration Experiment C.

The evidence from innumerable selective learning experiments, as typified by Demonstration Experiments A and B, leaves little doubt as to the soundness of the need-reduction generalization. This does not necessarily mean that the need-onset hypothesis is false; there may be more than one mechanism of reinforcement. While such seeming improvidence in biological economy appears somewhat opposed to the principle of parsimony, it is not without parallel in other fields; most organisms possess more than one means of excretion, and some organisms possess more than one independent means of

reproduction. Such general considerations merely pose the question and warn us of multiple possibilities; they cannot be decisive.

Turning to experimental evidence now available we find that selective learning of the type shown in Demonstration Experiments A and B usually yields results consistent only with the termination hypothesis. On the other hand, the results from conditioned-reflex experiments, typified by Demonstration Experiment C, are consistent with either hypothesis. This ambiguity probably arises from the brief duration of the shock usual in such experiments; the onset and termination of the need occur so close together that it is difficult clearly to distinguish the influence of each. For example, it is quite possible that the critical reinforcing factor in the conditioned kneejerk experiment (10) often cited in this connection (5, p. 85) may be the termination of the receptor discharge resulting from the usually rather severe blow on the patellar tendon, which is the unconditioned stimulus used to evoke this reaction. The matter is complicated still further by the intrusion of the generalized results of previous learning, especially where highly sophisticated human subjects are employed in the investigations. Thus the only critical evidence now available seems to favor the reduction or termination hypothesis; decision must probably come from carefully designed and executed experiments perhaps involving surgical interference with portions of the nervous system.

In the interim we shall proceed on the positive assumption that the termination of the need (or of its closely correlated receptor response) is a primary reinforcing factor; this hardly seems open to doubt. Even if the onset of the need, or of the correlated receptor response, proves to have genuine reinforcing capacity, the dynamics of behavior are such that it would not have much adaptive value.

## References

1. BECHTEREV, V. M. *General principles of human reflexology* (trans. by E. and W. Murphy from the Russian of the 1928 ed.). New York: International Publishers, 1932.

2. CANNON, W. B. *The wisdom of the body*. New York: W. W. Norton and Co., 1932.
3. DASHIELL, J. F. *Fundamentals of general psychology*. New York: Houghton Mifflin Co., 1937.
4. FINAN, J. L. Quantitative studies in motivation. I. Strength of conditioning in rats under varying degrees of hunger. *J. Comp. Psychol.*, 1940, 29, 119-134.
5. HILGARD, E. R., and MARQUIS, D. G. *Conditioning and learning*. New York: D. Appleton-Century Co., Inc., 1940.
6. HULL, C. L. Mind, mechanism, and adaptive behavior. *Psychol. Rev.*, 1937, 44, 1-32.
7. MURPHY, G. *A briefer general psychology*. New York: Harper and Brothers, 1935.
8. PAVLOV, I. P. *Conditioned reflexes* (trans. by G. V. Anrep). London: Oxford Univ. Press, 1927.
9. THORNDIKE, E. L. *The fundamentals of learning*. New York: Teachers College, Columbia Univ., 1932.
10. WENDT, G. R. An analytical study of the conditioned knee-jerk. *Arch. Psychol.*, 1930, 19, No. 123.

#### PRIMARY MOTIVATION AND REACTION POTENTIAL

It may be recalled that when the problem of primary reinforcement was under consideration (p. 68 ff.), the matter of organic need played a critical part in that the reduction of the need constituted the essential element in the process whereby the reaction was conditioned to new stimuli. We must now note that the state of an organism's needs also plays an important rôle in the causal determination of which of the many habits possessed by an organism shall function at a given moment. It is a matter of common observation that, as a rule, when an organism is in need of food only those acts appropriate to the securing of food will be evoked, whereas when it is in need of water, only those acts appropriate to the securing of water will be evoked, when a sexual hormone is dominant only those acts appropriate to reproductive activity will be evoked, and so on. Moreover, the extent or intensity of the need determines in large measure the vigor and persistence of the activity in question.

By common usage the initiation of learned, or habitual,

patterns of movement or behavior is called *motivation*. The evocation of action in relation to secondary reinforcing stimuli or *incentives* will be called *secondary motivation*; a brief discussion of incentives was given above in connection with the general subject of amount of reinforcement. The evocation of action in relation to primary needs will be called *primary motivation*; this is the subject of the present chapter.

### *The Empirical Roles of Habit Strength and Drive in the Determination of Action*

Casual observations such as those cited above often give us valuable clues concerning behavior problems, but for precise solutions, controlled quantitative experiments usually are necessary. In the present context we are fortunate in having an excellent empirical study which shows the functional dependence of the persistence of food-seeking behavior jointly on (1) the number of reinforcements of the habit in question, and (2) the number of hours of food privation. Perin (12) and Williams (20) trained albino rats on a simple bar-pressing habit of the Skinner type, giving separate groups different numbers of reinforcements varying from 5 to 90 under a standard 23 hours' hunger. Later the groups were subdivided and subjected to experimental extinction with the amount of food privation varying from 3 to 22 hours.

The gross outcome of this experiment is shown in Figure 1, where the height of each column represents the relative mean number of unreinforced reactions performed by each group before experimental extinction yielded a five-minute pause between successive bar pressures. The positions of the twelve columns on the base shows clearly the number of reinforcements and the number of hours' food privation which produced each. It is evident from an examination of this figure that *both* the number of reinforcements and the number of hours of food privation are potent factors in determining resistance to experimental extinction. Moreover, it is clear that for any given amount of food privation, e.g., 3 or 22 hours, the different numbers of reinforce-

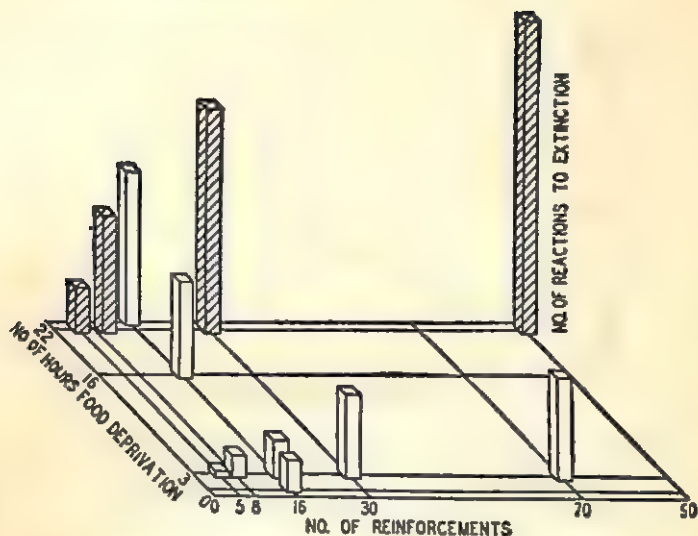


Fig. 1. Column diagram of the Perin-Williams data showing quantitatively how the resistance to experimental extinction in albino rats varies jointly with the number of reinforcements and the number of hours of food privation at the time the extinction occurred. The cross-hatched columns represent the groups of animals reported by Williams (20); the non-hatched columns represent the groups reported by Perin. (Figure reproduced from Perin, 12, p. 106.)

ments yield a close approximation to a typical positive growth function. On the other hand, it is equally clear that for a given number of reinforcements, e.g., 16, the number of hours of food privation has an almost linear functional relationship to the resistance to experimental extinction.

For a more precise analysis of these functional relationships it is necessary to fit two-dimensional curves to the data. The results of this procedure are presented in Figures 2 and 3. Figure 2 shows that resistance to extinction at the 16-reinforcement level is a slightly positively accelerated function of the number of hours'

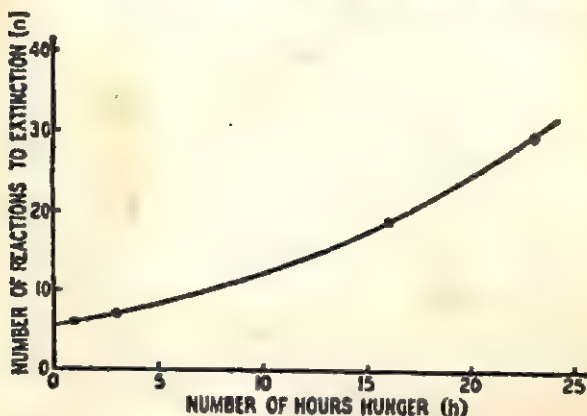


Fig. 2. Graphic representation of the data showing the systematic relationship between the resistance to experimental extinction (circles) and the number of hours' food privation where the number of reinforcements is constant at 16. The smooth curve drawn through the sequence of circles represents the slightly positively accelerated function fitted to them. This function is believed to hold only up to the number of hours of hunger employed in the original habit formation process: in the present case, 23. (Figure adapted from Perin, 12, p. 104.)

food privation for the first 22 hours. Figure 3 shows that a positive growth function fits both "learning" curves fairly well. An examination of the equations which generated these curves reveals that the asymptotes differ radically, clearly being increasing functions of the number of hours of food privation, but that the rates at which the curves approach their respective asymptotes are practically identical ( $F$  equals approximately  $1/25$  in both cases). Finally it may be noted that both curves, when extrapolated backward to where the number of reinforcements would equal zero, yield a *negative* number of extinctive reactions amounting to approximately four. This presumably is a phenomenon of the reaction threshold which will be discussed in some detail later; it is believed to mean that a habit strength sufficient to

resist four extinction reactions is necessary before reaction will be evoked by the stimuli involved.

For a final examination of the outcome of the experiment as a whole, the curves shown in Figures 2 and 3 were synthesized in such a way as to yield a surface fitted to the tops of all the columns of Figure 1. This surface is shown in Figure 4. An examination of this figure reveals the important additional fact that when the surface is extrapolated to where the number of hours' food privation is zero, the resistance to experimental extinction presumably will still show a positive growth function with  $n$ -values of considerable magnitude. As a matter of fact, the asymptote of the growth function where  $h = 0$  (satiation) is 28 per cent of that where  $h = 22$  hours.

These last results are in fairly good agreement with

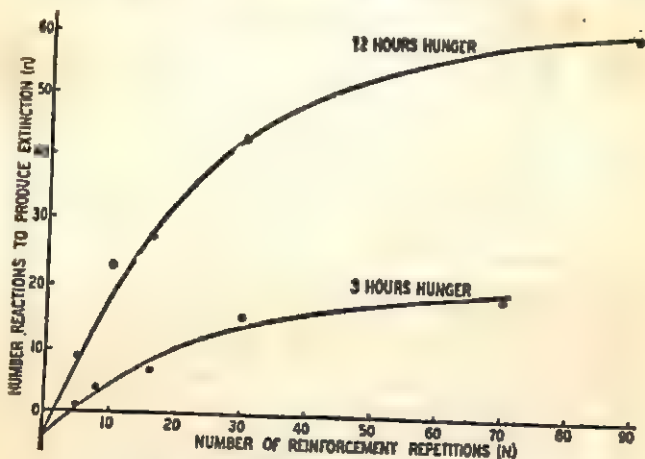


Fig. 3. Graphic representation of the two "learning" curves of Figure 1, shown in the same plane to facilitate comparison. The solid circles represent the empirical values corresponding to the heights of the relevant columns of Figure 1; the one hollow circle represents a slightly interpolated value. The smooth curves drawn among each set of circles represent the simple growth functions fitted to each set of empirical data. (Figure adapted from Perin, 12, p. 101.)

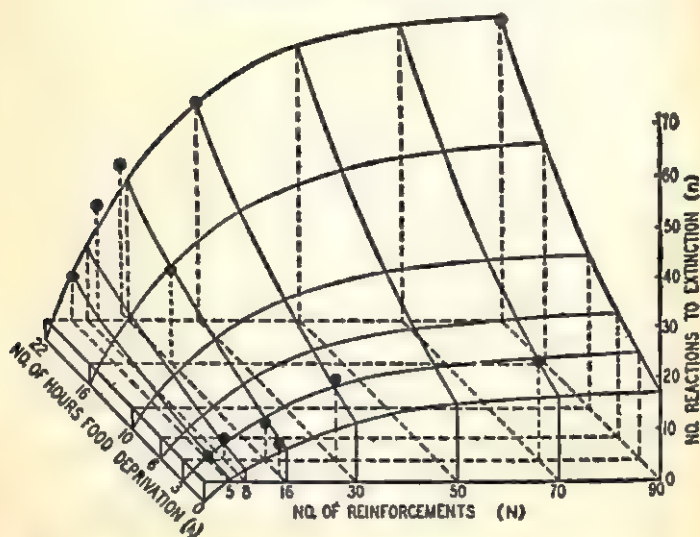


Fig. 4. Three-dimensional graph representing the fitted "surface" corresponding quantitatively to the action of the number of reinforcements and the number of hours of food privation following satiation, in the joint determination of the number of unreinforced acts of the type originally conditioned which are required to produce a given degree of experimental extinction. (Figure adapted from Perin, 12, p. 108.)

comparable values from several other experimental studies. Measurements of one of Skinner's published graphs, reproduced as Figure 5, indicate that his animals displayed approximately 17 per cent as much food-seeking activity at satiation as at 25 hours' food privation. Finch (3) has shown that at satiation a conditioned salivary reaction in nine dogs yielded a mean of 24 per cent as much secretion as was yielded at 24 hours' food privation. Similarly, Zener (22) reports that the mean salivary secretion from four dogs average at satiation 24 per cent as much as at from 21 to 24 hours' food privation. The considerable amounts of responsiveness to the impact of conditioned stimuli when the organism is in a state of food satiation may accordingly be considered as well established.

The continued sexual activity of male rats for some months after castration points in the same direction. Stone (15) reports that male rats which have copulated either shortly before or shortly after castration, when an adequate supply of hormone would be present, continue to show sexual behavior sometimes as long as seven or eight months after removal of the testes. According to Moore *et al.* (10), Stone (15), and Beach (1), this operation removes within 20 days not only the source of

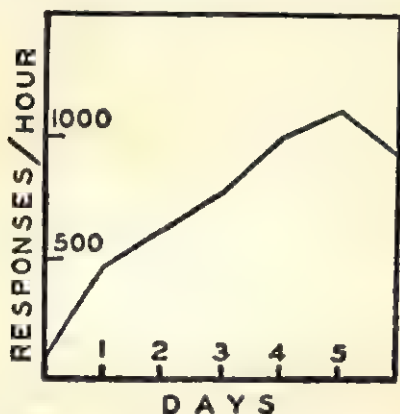


Fig. 5. Graph showing the relationship of the action potentiality as a function of the length of food deprivation following satiation. First note the fact that there is an appreciable amount of action potentiality at the beginning of this graph, where the amount of food deprivation is zero. Next, observe that the curve is relatively high at one day of food deprivation, which was the degree of drive under which the original training occurred. Finally, note that the rise in action potentiality is fairly continuous up to about five days, after which it falls rather sharply. This fall is evidently due to exhaustion, as the animals died soon after. The function plotted as the smooth curve of Figure 49 corresponds only to the first section of the present graph and clearly does not represent the functional relationship beyond a point where the number of hours of food deprivation is greater than 23. (Figure reproduced from Skinner, 13, p. 396.)

testosterone but, through the resulting atrophy of accessory glands, also the source of other specifically supporting secretions. A few weeks after castration, therefore, when the normal supply of sex hormones in the animal's body has been exhausted, the sex drive is presumably in about the same state as is the food drive after complete food satiation. The continued sexual activity of these animals thus presents a striking analogy to the continued operation of the food-release bar by Perin's rats after food satiation. While not absolutely convincing, this evidence from the field of sexual behavior suggests that the performance of learned reactions to moderate degrees in the absence of the specific drive involved in their original acquisition may be sufficiently general to apply to all primary motivational situations.

Closely related to this same aspect of Perin's investigation is a study reported by Elliott (2). Albino rats were trained in a maze under a thirst drive with water as the reinforcing agent until the true path was nearly learned, when the drive was suddenly shifted to hunger and the reinforcing agent to food. The outcome of this procedure is shown in Figure 6. There it may be seen that on the first trial under the changed condition of drive there was an appreciable disturbance of the behavior in the form of an increase in locomotor time; there was also an increase, of about the same proportion, in blind-alley entrances. On the later trials, however, the learning process appeared to proceed much as if no change had been made in the experimental conditions.

As a final item in this series there may be mentioned an empirical observation of Pavlov concerning the effect on an extinguished conditioned reaction of increasing the drive. On the analogy of Perin's experiment, it might be expected that this would again render the reaction evocable by the stimulus; and this in fact took place. In this connection Pavlov remarks (11, p. 127):

To illustrate this last condition we may take instances of differential inhibitions established on the basis of an alimentary reflex. If, for example, the dog has been kept entirely without food for a much longer period than usual before the experiment is conducted, the increase in excitability of the whole alimentary nervous mecha-

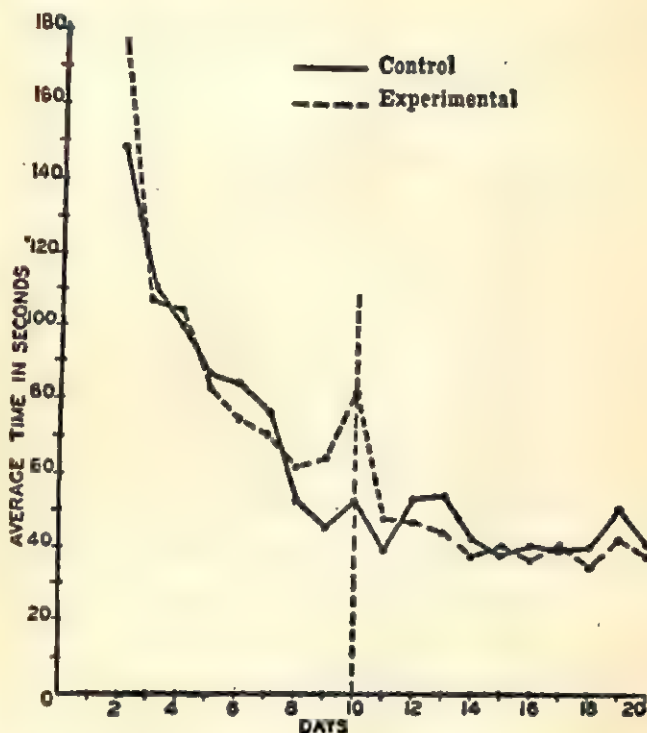


Fig. 6. Graphs showing the disruptive influence on a maze habit set up in albino rats on the basis of a water reinforcement, of having the drive (on the tenth day) suddenly shifted from thirst to hunger. (Reproduced from Elliott, 2, p. 187.)

nism renders the previously established differential inhibition wholly inadequate.

#### *Empirical Differential Reactions to Identical External Environmental Situations on the Basis of Distinct Drives*

A second important type of motivational problem was broached in an experiment reported by Hull (6).

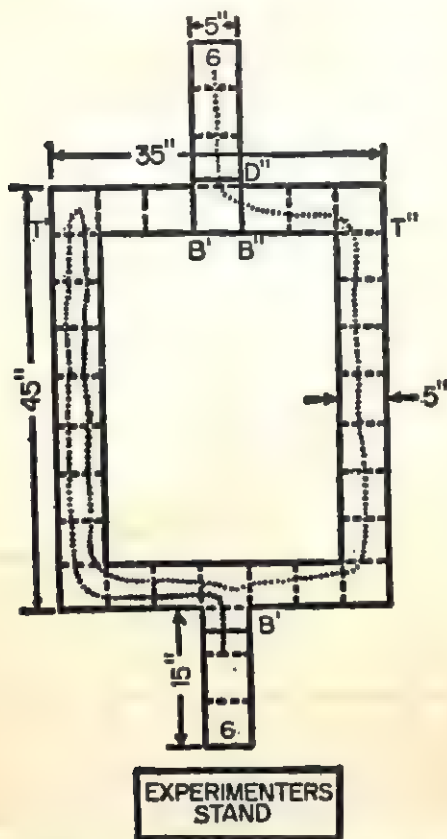


Fig. 7. Diagram of the maze employed in Hull's differential drive experiment. S = starting chamber; G = food chamber; D', D'' = doors manipulated by cords from the experimenter's stand; B', B'' = barriers across passageway, one of which was always closed. The course pursued by a typical rat on a "false" run is shown by the sinuous dotted line. Note that the animal went down the "wrong" side of the maze far enough to see the closed door at B' and then turned around. (Reproduced from Hull, 6.)

Albino rats were trained in the rectangular maze shown in Figure 7. On some days a given animal would be run in the maze when satiated with water, but with 23 hours' food privation, whereas on other days the same rat would be run when satiated with food but with 23 hours of water privation. The two types of days alternated according to a predetermined irregularity. On the food-privation days the reinforcement chamber always contained food and the left entrance, say, to the chamber was blocked so that access could be had only by traversing the right-hand side of the rectangle. On the water-privation days the reinforcement chamber always contained water, and the right-hand entrance to the reinforcement chamber would be blocked so that access to the water could be had only by traversing the left-hand side of the rectangle. The outcome of this experiment is shown in Figure 8. There it may be seen that while learning was very slow, the animals of the experimental group gradually attained a considerable power of making the reaction which corresponded to the drive dominant at the time.

The capacity of rats to learn this type of discrimination was later demonstrated more strikingly by Leeper (8), in a substantially similar investigation. Leeper's experiment differed, however, in the detail that two distinct reinforcement chambers were employed and no passageways were blocked at any time, so that if on a "food" day the rat went to the water side he always found water, and if on a "water" day he went to the food side, he always found food. Under these conditions the animals learned to perform the motivational discrimination with great facility; Leeper's animals needed only about one-twelfth the number of trials required by the original Hull technique, though again the process of acquisition was gradual.<sup>2</sup>

\* This striking difference is attributed in part to the operation of spatial orientation and in part to the fact that when rats are deprived of either food or water they do not consume a normal amount of the other substance; this prevents genuine satiation of the supposed satiated drive. For example, thirsty rats supposedly satiated with food will, after receiving even a few drops of water, very generally eat if food is available (6, p. 270); and

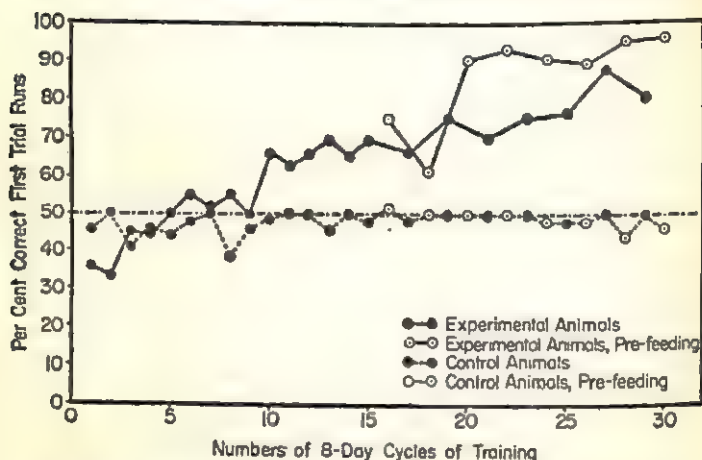


Fig. 8. Composite graphs showing the per cent of correct choices at the first trial of each experimental day in the discrimination by rats between hunger and thirst motivation.

### *Does the Principle of Primary Stimulus-Intensity Generalization Apply to the Drive Stimulus ( $S_D$ )?*

A factor with considerable possible significance for the understanding of motivation is the relationship between the degree of similarity of the need at the time of reinforcement and that at the time of extinction, on the one hand, and the associated resistance to experimental extinction on the other. No specific experiments have been found bearing exactly on this point, but several incidental and individually inconclusive bits of evidence may be mentioned as indicating the general probabilities of the situation.

The first of these was reported by Heathers and Arakelian (4). Albino rats were trained to secure food pellets by pressing a bar in a Skinner-Ellson apparatus.

rats, like humans, frequently drink while eating dry food if water is available. Thus after the first trial Leeper's animals were presumably operating under both drives, and one drive or the other was reinforced no matter which path was traversed.

Next, half of the animals were partially extinguished under a weak hunger, and the remainder were extinguished to an equal extent under a strong hunger. Two days later the animals were subjected to a second extinction, half of each group under the same degree of hunger as in the first extinction, and the remaining half under a drive equal to the first-extinction hunger of the other group. Combining the state of food privation of the first and second extinctions, there were thus four hunger-extinction groups:

- 1, strong-strong; 2, strong-weak;
- 3, weak-strong; 4, weak-weak.

The authors report that a statistical pooling of the results from these four groups of animals revealed a tendency of the rats extinguished twice on the same drive to resist extinction less than did those animals which were extinguished the second time on a drive different from that employed on the first occasion. In two independent studies this difference amounted to approximately 4 and 6 per cent respectively; the latter results are reported to have a probability of 8 in 10 that the difference was not due to chance. This experimental outcome is evidently related to the primary generalization of stimulus intensity and suggests that *perseverative extinction effects are to some extent specific to the primary drive or need intensity under which the extinction occurs.*

By analogy, the stimulus-intensity generalization gradient apparently found in the case of extinction effects just considered strongly suggests the operation of the same principle in the case of reinforcement effects. Now, such a gradient has been demonstrated experimentally by Hovland (see p. 186 ff.); it naturally has its greatest value at the point of reinforcement. Consequently it is to be expected that in a curve of motivation intensity such as that of Skinner (shown in Figure 5) a special elevation or inflection would appear at the drive intensity at which the original reinforcement occurred. Whether a mere coincidence arising from sampling errors or not, exactly such an inflection may be seen in Skinner's empirical graph at one day of food privation, which was

in fact the drive employed by Skinner in the training of the animals in question. The present set of assumptions implies that if Skinner's curve as shown in Figure 5 were to be plotted in detail by hours rather than days, it would present a positive acceleration from zero to one day of food privation. Now, Perin's study did plot this region in some detail, and Figure 2 shows that a positively accelerated function was found. These facts still further increase the probability that the principle of stimulus-intensity generalization applies to the drive stimulus ( $S_D$ ).

### *The Influence of Certain Drugs on Experimental Extinction and Its Perseverational Effects*

Certain drugs are known to influence markedly the phenomena of experimental extinction. Switzer (18) investigated the effect of caffeine citrate on the conditioned galvanic skin reaction in human subjects, using a control dose of milk sugar. He found that caffeine increased resistance to experimental extinction; incidentally he also found that caffeine increased the amplitude of the unconditioned galvanic skin reaction and decreased the reaction latency.

Pavlov (11, p. 127) reported a somewhat related experiment performed by Nikiforovsky. An alimentary salivary conditioned reflex had been set up to a tactile stimulus on a dog's forepaw. This reaction tendency generalized to other parts of the animal's skin, including a point on the back which subsequently was completely extinguished. At the latter stage of training the stimulus on the paw yielded five drops of saliva during the first minute of stimulation, whereas stimulation of the extinguished spot on the back yielded a zero reaction. Thereupon, the animal was given a subcutaneous injection of 10 c.c. of 1 per cent solution of caffeine. A few minutes later the stimulus when applied to the forepaw evoked four drops during the first minute, and when applied to the previously extinguished spot on the back, yielded three drops (11, p. 128), thus indicating a major dissipation of the extinction effects.

Miller and Miles (9) have contributed to this field.

They demonstrated in albino rats traversing a 25-foot straight, enclosed runway that an injection of caffeine sodio-benzoate reduced the locomotor retardation due to experimental extinction by about two-thirds. In the same study it was shown that the retardation in locomotor time due to satiation was reduced by the caffeine solution approximately one-half (9).

Benzedrine is another substance which when thrown into the blood stream has the power of greatly retarding the onset of experimental extinction. This was demonstrated by Skinner and Heron (14) to hold for the Skinner bar-pressing habit.

### *Sex Hormones and Reproductive Activity*

As a final set of empirical observations concerning motivation we must consider briefly the relation of sex hormones to reproductive behavior. Within recent years an immense amount of excellent experimental work has been performed in this field, though only brief notice of it can be taken in this place. An account of two typical bits of this work was given above. In a recent comprehensive summary by Beach (1) the following propositions appear to have fairly secure empirical foundation:

1. Animals of practically all species which through castration have become sexually unresponsive to ordinary incentive stimulation, become responsive promptly on the injection of the appropriate hormone—usually testosterone propionate for males and estrogen for females.

2. Presumptively normal male rats differ greatly in their sexual responsiveness, all the way from those which will attempt copulation with inanimate objects to those which will not react even to an extremely receptive and alluring female. The injection of testosterone usually raises the reactivity of all but a few of the most sluggish animals. Alternatively, the presentation of an especially attractive incentive tends to have the same objective effect, though to a lesser degree (17).

3. Destruction of the cerebral cortex decreases sexual reactivity roughly in proportion to the extent of such destruction, very much as occurs in the case of food

S.C.E.R.T., West Bengal.

Date..... 12.3.69

3102

154.4

TEE

habits. If destruction has not been too great, injection of the hormone will largely restore sexual responsiveness to appropriate incentives. The presentation of an exceptionally attractive incentive will, however, have much the same effect upon the objective behavior of such organisms.

4. Virgin male organisms which are unresponsive to an ordinary receptive female, after a few copulations under the influence of an injection of the hormone will remain responsive long after the hormone has presumably disappeared from the animal's body. This is believed to be caused by the learning resulting from the incidental reinforcement which occurred when the animal was under the influence of the hormone (1).

5. Many intact individuals of both sexes in most species occasionally manifest a portion of the behavior pattern characteristic of the opposite sex. Injection of the sex hormone of the opposite sex in castrated individuals of either sex tends strongly to the evocation of the sexual behavior pattern characteristic of normal organisms of the opposite sex on appropriate stimulation; this, however, is not usually as complete as the gross anatomical equipment of the organisms would seem to permit. Curiously enough, large doses of testosterone given to male rats make possible the elicitation of all elements of the typical female sexual behavior (1).

### *Primary Motivational Concepts*

With the major critical phenomena of primary motivation<sup>3</sup> now before us, we may proceed to the attempt to formulate a theory which will conform to these facts.

At the outset it will be necessary to introduce two notions not previously discussed. These new concepts are analogous to that of habit strength ( ${}_sH_R$ ) which, it will be recalled (p. 114), is a logical construct conceived in the quantitative framework of a centigrade system.

<sup>3</sup> The empirical phenomena of secondary motivation, including such matters as incentive, fractional anticipatory goal- and subgoal-reactions, cannot be treated in the present volume because space is not available.

The first of the two concepts is *strength of primary drive*; this is represented by the symbol  $D$ . The strength-of-drive scale is conceived to extend from a zero amount of primary motivation (complete satiation) to the maximum possible to a standard organism of a given species. In accordance with the centigrade principle this range of primary drive is divided into 100 equal parts or units. For convenience and ease of recall, this unit will be called the *mote*, a contraction of the word *motivation* with an added *e* to preserve normal pronunciation.

Because of the practical exigencies of exposition the second of the new concepts has already been utilized occasionally in the last few pages, where it has been referred to as the "reaction tendency," a term in fairly general use though lacking in precision of meaning. For this informal expression we now substitute the more precise equivalent, *reaction-evocation potentiality*; or, more briefly, *reaction potential*. This will be represented by the symbol  ${}_sE_R$ . Like habit ( ${}_sH_R$ ) and drive ( $D$ ), reaction-evocation potential is also designed to be measured on a 100-point scale extending from a zero reaction tendency up to the physiological limit possible to a standard organism. The unit of reaction potentiality will be called the *wat*, a contraction of the name *Watson*.

It should be evident from the preceding paragraphs that  $D$  and  ${}_sE_R$  are symbolic constructs in exactly the same sense as  ${}_sH_R$  (see p. 111 ff.), and that they share both the advantages and disadvantages of this status. The drive concept, for example, is proposed as a common denominator of all primary motivations, whether due to food privation, water privation, thermal deviations from the optimum, tissue injury, the action of sex hormones, or other causes. This means, of course, that drive will be a different function of the objective conditions associated with each primary motivation. For example, in the case of hunger the strength of the primary drive will probably be mainly a function of the number of hours of food privation, say; in the case of sex it will probably be mainly a function of the concentration of a particular sex hormone in the animal's blood; and so on. Stated formally,

$$D = f(h)$$

$$D = f(c)$$

$$D = \text{etc.},$$

where  $h$  represents the number of hours of food privation of the organism since satiation, and  $c$  represents the concentration of a particular hormone in the blood of the organism.

Turning now to the concept of reaction-evocation potentiality, we find, thanks to Perin's investigation sketched above (p. 227 ff.), that we are able at once to define  ${}_sE_R$  as the product of a function of habit strength ( ${}_sH_R$ ) multiplied by a function of the relevant drive ( $D$ ). This multiplicative relationship is one of the greatest importance, because it is upon  ${}_sE_R$  that the amount of action in its various forms presumably depends. It is clear, for example, that it is quite impossible to predict the vigor or persistence of a given type of action from a knowledge of either habit strength or drive strength alone; this can be predicted only from a knowledge of the product of the particular functions of  ${}_sH_R$  and  $D$  respectively; in fact, this product constitutes the value which we are representing by the symbol  ${}_sE_R$ .

### *Summary and Preliminary Physiological Interpretation of Empirical Findings*

Having the more important concepts of the systematic approach of primary motivation before us, we proceed to the formulation of some empirical findings as related to motivation.

Most, if not all, primary needs appear to generate and throw into the blood stream more or less characteristic chemical substances, or else to withdraw a characteristic substance. These substances (or their absence) have a selective physiological effect on more or less restricted and characteristic portions of the body (e.g., the so-called "hunger" contractions of the digestive tract) which serves to activate resident receptors. This receptor activation constitutes the drive stimulus,  $S_D$ . In the case of tissue injury this sequence seems to be reversed; here the energy producing the injury is the drive

stimulus, and its action causes the release into the blood of adrenal secretion which appears to be the physiological motivating substance.

It seems likely, on the basis of various analogies, that, other things equal, the intensity of the drive stimulus would be some form of negatively accelerated increasing function of the concentration of the drive substance in the blood. However, for the sake of expository simplicity we shall assume in the present preliminary analysis that it is an increasing linear function.

The afferent discharges arising from the drive stimulus ( $S_D$ ) become conditioned to reactions just the same as any other elements in stimulus compounds, except that they may be somewhat more potent in acquiring habit loadings than most stimulus elements or aggregates. Thus the drive stimulus may play a rôle in a conditioned stimulus compound substantially the same as that of any other stimulus element or aggregate. As a stimulus,  $S_D$  naturally manifests both qualitative and intensity primary stimulus generalization in common with other stimulus elements or aggregates in conditioned stimulus compounds.

It appears probable that when blood which contains certain chemical substances thrown into it as the result of states of need, or which lacks certain substances as the result of other states of need, bathes the neural structures which constitute the anatomical bases of habit ( ${}_sH_R$ ), the conductivity of these structures is augmented through lowered resistance either in the central neural tissue or at the effector end of the connection, or both. The latter type of action is equivalent, of course, to a lowering of the reaction threshold and would presumably facilitate reaction to neural impulses reaching the effector from any source whatever. As Beach (1) suggests, it is likely that the selective action of drives on particular effector organs in non-learned forms of behavior acts mainly in this manner. It must be noted at once, however, that sensitizing a habit structure does not mean that this alone is sufficient to evoke the reaction, any more than that caffeine or benzedrine alone will evoke reaction. Sensitization merely gives the relevant neural tissue, upon the occurrence of an adequate set of recep-

tor discharges, an augmented facility in routing these impulses to the reactions previously conditioned to them or connected by native (inherited) growth processes. This implies to a certain extent the undifferentiated nature of drive in general, contained in Freud's concept of the "libido." However, it definitely does not presuppose the special dominance of any one drive, such as sex, over the other drives.

While all drives seem to be alike in their powers of sensitizing *acquired* receptor-effector connections, their capacity to call forth within the body of the organism characteristic and presumably distinctive drive stimuli gives each a considerable measure of distinctiveness and specificity in the determination of action which, in case of necessity, may be sharpened by the process of patterning to almost any extent that the reaction situation requires for adequate and consistent reinforcement. In this respect, the action of drive substances differs sharply from that of a pseudo-drive substance such as caffeine, which appears to produce nothing corresponding to a drive stimulus.

Little is known concerning the exact quantitative functional relationship of drive intensity to the conditions or circumstances which produce it, such as the number of hours of hunger or the concentration of endocrine secretions in the blood. Judging from the work of Warden and his associates (19), the relationship of the hunger drive up to two or three days of food privation would be a negatively accelerated increasing function of time, though a study by Skinner (Figure 5) suggests that it may be nearly linear up to about five days. For the sake of simplicity in the present explorational analysis we shall assume the latter as a first approximation.

Physiological conditions of need, through their sensitizing action on the neural mediating structures lying between the receptors and the effectors ( $sH_R$ ), appear to combine with the latter to evoke reactions according to a multiplicative principle, i.e., reaction-evocation potentiality is the product of a function of habit strength multiplied by a function of the strength of drive:

$${}_sE_R = f({}_sH_R) \times f(D).$$

In the next section it shall be our task to consider in some detail what these functions may be; if successful we shall then possess the main portion of a molar theory of primary motivation.

### *The Quantitative Derivation of ${}_sE_R$ From ${}_sH_R$ and $D$*

Since we have taken Perin's experiment as our main guide in the analysis of the primary motivational problem in general, it will be convenient to take the need for food as the basis for the detailed illustration of the working of the molar theory of motivation; this we now proceed to develop.

Turning first to the habit component of  ${}_sE_R$ , we calculate the values of  ${}_sH_R$  as a positive growth function; we use in this calculation the fractional incremental value ( $F$ ) found by Perin to hold for the learning processes represented in Figure 3, which was approximately  $1/25$  for each successive reinforcement. On this assumption the values at various numbers of reinforcements, e.g., 0, 1, 3, 9, 18, 36, and 72, have been computed. These are shown in column 2 of Table 1.

The habit-strength values of column 2, Table 1, consist of the physiological summation of the habit-strength loadings of the stimulus components, represented by the original drive stimulus  $S_D'$  and the non-drive components, which we shall represent by  $S_I$ . Assuming as a matter of convenience that  $S_D'$  and  $S_I$  have equal loadings, the value of each (see fifth terminal note) is easily calculated for the several numbers of reinforcements. These values are shown in column 3 of Table 1.

Turning next to the matter of drive, it will be assumed that the original learning took place under a 24-hour food privation. Assuming further that drive is a linear function of the number of hours' hunger and that (Figure 5) the maximum of 100 motes would be reached at five days or 120 hours. Perin's periods of food privation may be converted into units of drive strength by multiplying the number of hours' food

TABLE 1

TABLE SHOWING THE PRELIMINARY STEPS IN THE DERIVATION OF A SERIES OF THEORETICAL REACTION-POTENTIAL VALUES FROM A VARIED SET OF ANTECEDENT REINFORCEMENTS UNDER A DRIVE OF 20 UNITS' STRENGTH, THE RESULTING HABITS BEING EVALUATED FOR REACTION POTENTIALITY AT DRIVE-STRENGTHS OF 0.00, 2.50, 6.667, 13.333, AND 20.0 UNITS.

N	Habit Strength as Formed		Effective Habit Strength of Drive-Stimulus Component ( $s_D \bar{H}_R$ ) Derived from Intensity Generalization Gradient						Physiological Summation of Habit Strengths of Both Components ( $s_1 + s_D \bar{H}_R$ )					Reaction-Evocation Potential ( $s_{ER}$ ) as Mediated by Various Strengths of Drive				
	Two Com- ponents $s_1 + s_D \bar{H}_R$	One Com- ponent ( $s \bar{H}_R$ )	$d' =$ 20.00	$d' =$ 17.5	$d' =$ 13.33	$d' =$ 6.67	$d' =$ 0.00	$D =$ 0.00	$D =$ 2.50	$D =$ 6.67	$D =$ 13.33	$D =$ 20.00	$D =$ 0.00	$D =$ 2.50	$D =$ 6.67	$D =$ 13.33	$D =$ 20.00	
			4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	
0	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	
1	3.05	1.54	1.03	1.08	1.18	1.35	1.54	2.55	2.60	2.70	2.87	3.06	.23	.30	.41	.61	.83	
3	8.77	4.49	3.00	3.15	3.43	3.92	4.49	7.36	7.50	7.77	8.23	8.78	.67	.85	1.18	1.75	2.39	
9	23.35	12.46	8.31	8.75	9.54	10.89	12.46	19.73	20.12	20.81	21.99	23.37	1.79	2.29	3.15	4.66	6.37	
18	39.43	22.18	14.81	15.58	16.95	19.38	22.18	33.71	34.30	35.37	37.26	39.44	3.06	3.90	5.36	7.90	10.76	
36	58.13	35.29	23.56	24.78	26.96	30.84	35.29	50.54	51.33	52.74	55.25	58.13	4.59	5.83	7.99	11.72	15.85	
72	71.21	46.34	30.94	32.54	35.40	40.50	46.34	62.94	63.80	65.34	68.07	71.21	5.72	7.25	9.90	14.44	19.42	

privation by the fraction 100/120. In this way we secure the following drive or  $D$ -values:

Number of hours' food privation ( $h$ ):	0	3	8	16	24
Strength of drive in motes ( $D$ ):	0	2.5	6.667	13.333	20
Deviation ( $d$ ) of possible $D$ 's from the drive ( $D'$ ) of original learning:	20	17.5	13.333	6.667	0

Now,  $S_D$  is assumed to be approximately a linear function of  $D$ . It follows from this and the principle of primary stimulus generalization that action evoked under any other intensity of drive (and drive stimulus) than that involved in the original habit formation must be subject to primary intensity-stimulus generalization. Assuming the relatively flat gradient yielded by an  $F$ -value of 1/50, it is easy to calculate the value of  $_{SD}H_R$  at each degree of the five  $D$ -values taken above. These  $_{SD}H_R$  values are shown in columns 4, 5, 6, 7, and 8 respectively of Table 1. A glance at the bottom entries of each of the columns shows that the values of  $_{SD}H_R$  fall progressively from 46.34 at  $D = 20$  (i.e.,  $d = 0.00$ ) to 30.94 at  $D = 0$  (i.e.,  $d = 20$ ).

We must now combine these habit values by the process of physiological summation characteristic of conditioned stimulus compounds (neglecting the effects of afferent interaction) with the habit loading of the non-drive stimulus component of the compound which is represented by the values appearing in column 3. The physiological summation of the values in column 3 with the values of columns 4 to 8 gives us the habit-strength values shown in columns 9, 10, 11, 12, and 13 of Table 1. It will be noticed that this final recombination of the  $_sH_R$  values where  $D = 20$  yields exactly the same values as those of column 2. This is because when reaction evocation occurs at the original drive ( $D'$ ), i.e., where  $D = D'$ , no distortion of the  $S_D$  component of the habit results, the synthesis being exactly the reverse of the analysis which took place between columns 2 and 3.

With the theoretical values of  $f(_sH_R)$  available in

columns 9 to 13 inclusive of Table 1, we may now turn our attention to the problem of  $f(D)$ . It is assumed that  $D$  itself acts upon  ${}_sH_R$  as a direct proportion. However, there is the complication that other or alien drives active at the time (represented in the aggregate by the symbol  $\bar{D}$ ) have the capacity to sensitize habits not set up in conjunction with them. Let it be supposed that this generalized effect of alien drives adds 10 points to the actual drive throughout the present situation. Thus the effective drive ( $\bar{D}$ ) operative on a given habit would necessarily involve the summation of  $\bar{D}$  and  $D$ ; in the case of the 24-hour food privation a simple summation would in the present situation amount to  $10 + 20$ , or 30, and at 120 hours it would be  $10 + 100$ , or 110. In order to maintain our centigrade system the simple summation must be divided by the maximum possible under these assumptions, or 110. Accordingly we arrive at the formula,

$$\bar{D} = 100 \frac{\bar{D} + D}{\bar{D} + 100},$$

where  $D$  represents the *effective drive* actually operative in producing the reaction potential.

Now, assuming that reaction evocation potentiality is essentially a multiplicative function of habit strength and drive, i.e., that,

$${}_sE_R = f({}_sH_R) \times f(D),$$

since  $f({}_sH_R)$  is  ${}_sH_R$ , and  $f(D)$  is  $\bar{D}$ , we have by substitution,

$${}_sE_R = {}_sH_R \times \bar{D}.$$

However, since both  ${}_sH_R$  and  $\bar{D}$  are on a centigrade scale, their simple product would yield values on a ten-thousand point scale; therefore, to keep  ${}_sE_R$  also to a centigrade scale we write the equation,

$${}_sE_R = \frac{{}_sH_R \times \bar{D}}{100}.$$

Substituting the equivalent of  $\bar{D}$  and simplifying, we have as our final equation,

$${}_sE_R = {}_sH_R \frac{{}_sH_R D + D}{\dot{D} + 100}$$

The second portion of this formula, with the various  $D$  values substituted, is,

$$\begin{array}{ccccc} \frac{10 + 0}{110} & \frac{10 + 2.5}{110} & \frac{10 + 6.667}{110} & \frac{10 + 13.333}{110} & \frac{10 + 20}{110} \\ = .0909 & .1136 & .1515 & .2121 & .2727. \end{array}$$

The values of  ${}_sE_R$  are accordingly obtained simply by multiplying the several entries of column 9 by .0909, those of column 10 by .1136, and so on. These products are presented in detail in columns 14, 15, 16, 17 and 18 of Table 1, which are the values we have been seeking; they are shown diagrammatically by the curved surface of Figure 9. A comparison of the theoretical values of Figure 9 with the surface fitted to the empirical values represented by the circles in Figure 4 indicate that the theoretical derivations approximate the facts very closely indeed.

Computations analogous to the preceding have shown that the present set of postulates and constants also hold when  $D > D'$  at least up to three days of food privation. The theoretical curve for all values of  $D$  between 0 and 72 hours yields a positively accelerated reaction potential up to 24 hours ( $D'$  in the present analysis), where there is a slight inflection; as  $D$  increases above  $D'$  there is at first a brief period of positive acceleration, which is followed by a protracted period that is nearly linear, the whole showing a fair approximation to Figure 5.

Generalizing from Table 5 and Figure 6, the following corollaries may be formulated as a kind of condensed summary of the implications of the present set of assumptions as shown by the preceding computations:

I. When *habit strength* is zero, *reaction-evocation potential* is zero.

II. When *primary drive strength* ( $D$ ) is zero, *reaction-*

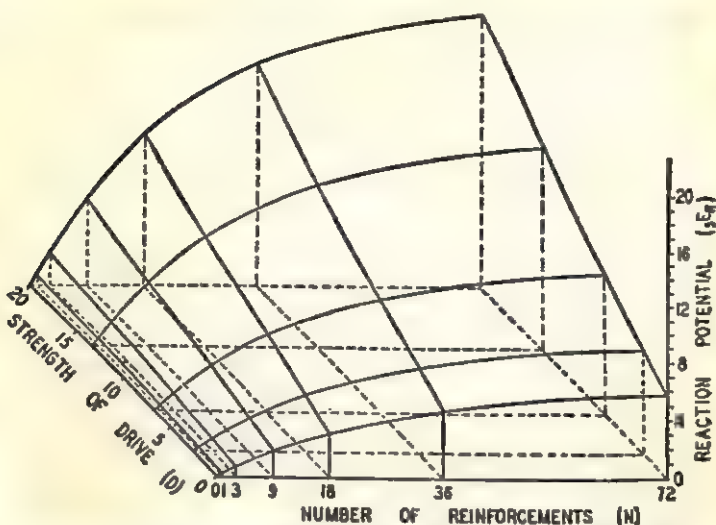


Fig. 9. Graphic representation of the theoretical joint determination of reaction potential by various numbers of reinforcements under a drive ( $D'$ ) of 20 units' strength when functioning under drives ( $D$ ) of various strengths less than that of the original habit formation. Note the detailed agreement with the comparable empirical results shown in Figure 4.

evocation potential ( ${}_sE_R$ ) has an appreciable but relatively low positive value which is a positive growth function of the number of reinforcements. Corollaries I and II both agree in detail with Perin's empirical findings.

As the drive ( $D$ ) increases from zero to  $D'$ :

III. The reaction-evocation potential increases with a slight positive acceleration.

IV. The reaction-evocation potential maintains its positive growth relationship to the number of reinforcements. Both of these corollaries agree in detail with Perin's empirical findings.

As the drive ( $D$ ) increases above  $D'$ :

V. There is a definite inflection in the  ${}_sE_R$  function at  $D$ , the slope for values of  $D$  just greater than  $D'$  being less than for those just below.

VI. *The reaction-evocation potential above  $D'$  increases at first with a slight positive acceleration, which soon gives place to a practically linear relationship.* Both of these corollaries agree in detail with Skinner's empirical findings (Figure 7).

*Miscellaneous Corollaries Flowing from the Present Primary Motivation Hypothesis*

The first problem in this series is that presented by Elliott's experiment described above, the outcome of which is clearly shown in Figure 6. Here we have the case of a reaction tendency set up on the basis of one drive, showing a partial but by no means complete disruption when this drive (thirst) is abruptly replaced by another drive, that of hunger. At this point we recall the assumption stated earlier that all drives alike are able to sensitize all habits. Applying this to the behavior of Elliott's animals during the first critical test trial on the maze after the change in drive, it is to be expected that while hunger was then the dominant drive, certain residual amounts of various other drives (including thirst) were also active. These in the aggregate ( $D$ ), the hunger drive included, are presumed to operate in a multiplicative manner upon the habit strength effective at the moment in determining reaction potentiality. It is assumed that this would be enough to evoke, on the average, about 20 per cent as much activity as is evoked by the thirst drive.

This means that the residual drive ( $\bar{D}$ ) must amount to considerably more than 20 per cent of the regular thirst drive, say. For example in the detailed analysis of the preceding section, where 24 hours' hunger stood at 20 units of drive, this residual drive was placed at 10 units, which is 50 per cent as much as 20. Nevertheless, the reaction potential at 24 hours' hunger came out at 19.42 units, whereas that at satiation or zero drive stood at 4.58 units, the latter being only about 23 per cent of the former. The explanation of the paradoxical difference of 50 versus 23 per cent is significant; it arises largely from the fact that when the hitherto dominant

drive ceases to be active, not only are there lost the 20 units of drive strength previously contributed by this need, but *there is also lost to the conditioned stimulus compound the sizable component made up by  $S_D$ , the withdrawal of which materially reduces the available habit strength associated with the situation in question, and so reduces the resulting reaction potential.*

On the basis of the above analysis we may formulate the following additions to the corollaries listed in the preceding section:

VII. *Under the conditions of the satiation of the dominant drive involved in the original habit-acquisition, there are sufficient residuals of other drives which in the aggregate yield on the average an excitatory potential amounting to around 20 per cent of that mobilized by a 24-hour hunger on a habit originally set up on the basis of this drive.*

VIII. *In case an organism is presented with all the stimuli characteristic of a habit, if the original drive is replaced by a strong second drive whose  $S_D$  activates no conflicting habit tendency, the reaction potential to the execution of the habitual act will be stronger than would be the case if the irrelevant second drive were not active.* This means that if a control group with both hunger and thirst thoroughly satiated were to be added to the Elliott experiment described above, the mean retardation in the running time and the mean number of errors would increase appreciably above what resulted from a mere replacement of one drive by another (6).

A second problem concerns the relation of the experimental extinction of a reaction tendency to the drive intensity operative at the time of extinction. Now, the passage from Pavlov quoted above strongly suggests that *experimental extinction effects are in some sense directly opposed to reaction potential rather than merely to habit strength.* Since with a constant habit strength an increase in the drive augments the reaction potential, and since extinction effects are an increasing function of the number of unreinforced evocations, it follows that:

IX. *The number of reinforcements being constant, the*

*stronger the relevant drive, the greater will be the number of unreinforced evocations which will be required to reduce the reaction potential to a given level.*

*X. The number of reinforcements being constant, the stronger an allied but irrelevant drive active at the time of extinction, the greater will be the number of unreinforced evocations required to reduce the reaction potential to a given level, though this number will be materially less than would be required under the same intensity of the relevant drive.*

Thus if a habit set up on the basis of a thirst drive were extinguished under a sizable hunger drive but with water satiation, the theory demands that the reaction potential would extinguish with fewer unreinforced evocations than would be the case under the same intensity of the thirst drive in conjunction with a zero hunger drive; moreover, such a habit would require more unreinforced reaction evocations to produce a given degree of extinction under a strong hunger drive than under a weak one. By the same type of reasoning it is to be expected that if a reaction tendency were set up in male rats under hunger or thirst, and if subsequently a random sample of the organisms were castrated, experimental extinction under a normal hunger drive would occur more quickly than it would in the non-castrated organisms.

At this point we turn to a more detailed consideration of Pavlov's observation just referred to, that when he had performed an experimental extinction under a given drive and then increased the drive, the conditioned stimulus would again evoke the reaction. This may be deduced rather simply: If a certain number of unreinforced evocations of a reaction have produced sufficient extinction effects to neutralize a given amount of excitatory potential, an increase in the drive will increase the excitatory potential which the existent extinction effects will no longer suffice to neutralize completely. The balance of the reaction potential will accordingly be available to evoke reaction and, upon adequate stimulation, will do so. We thus come to our eleventh corollary:

*XI. If a reaction tendency is extinguished by massed*

*reaction evocations under a given strength of drive, and if at once thereafter the drive is appreciably increased, the original stimulation will again evoke the reaction.*

Our final question concerns an exceedingly important problem in adaptive dynamics. It has already been pointed out that as a rule action sequences required to satisfy a food need are different from those required to satisfy a water need, and both would ordinarily be quite different from the acts which would be required to satisfy a sex drive. This problem is posed very sharply when, as in the Hull-Leeper experiments, an organism is presented with an identical objective situation and required to make a differential reaction purely on the basis of the need dominant at the moment. These experiments confirm everyday observations that animals can adapt successfully to such situations. The question before us is how this behavior is to be explained.

At first sight it might be supposed that in this situation the animals would merely associate  $S_h$  with turning to the right, say, and  $S_l$  with turning to the left, and that adaptation would thereby be complete. A little further reflection will show, however, that this simple explanation is hardly adequate, because if there were really an independent and functionally potent receptor-effector connection between the hunger-drive stimulus and turning to the right the animal would, when hungry, be impelled to turn to the right continuously when in its cage or wherever it happened to be, as well as at the choice point in the maze. The animals, of course, display no such behavior, any more than we ourselves do.

The present set of postulates mediates the explanation chiefly on the basis of a secondary process known as *patterning*. Unfortunately it will not be possible to give an exposition of this exceedingly important subject until a later chapter. However, pending the detailed presentation in that place we shall here merely indicate dogmatically the nature of patterning and briefly sketch the application of this secondary principle to the problem in adaptive dynamics now before us.

By the term "patterning" we mean the process whereby organisms acquire the capacity of reacting (or not reacting) to particular combinations of stimuli as

distinguished from the several component stimulus elements or aggregates making up the compound. At bottom this process turns out to be a case of learning to discriminate afferent interaction effects. Specifically, the principle of afferent interaction implies that in the Hull-Leeper studies afferent impulses ( $\check{s}$ ) arising from the environmental stimuli ( $S_1$ ) are somewhat different when stimulation occurs in combination with the hunger-drive stimulus ( $S_h$ ) from those which result from the same stimulation in combination with the thirst-drive stimulus ( $S_t$ ). Similarly, the afferent impulses arising from  $S_h$  and  $S_t$  are somewhat different when initiated in conjunction with  $S_1$  from those initiated by  $S_h$  and  $S_t$  in the cage or other situations. If the afferent impulses arising from the environmental stimuli uncomplicated by any particular drive be represented by  $s$ , then these impulses when modified by the interaction with the hunger-drive stimulus may be represented by  $\check{s}_h$ , and when modified by interaction with the thirst-drive by  $\check{s}_t$ . Since there are but two alternatives, it is to be expected that at the outset of training, reaction would be about 50 per cent correct. However, as the differential reinforcement yielded by the techniques employed in these investigations continues, the gradient of generalization between  $\check{s}_h$  and  $\check{s}_t$  would progressively steepen, i.e., discrimination learning would gradually take place, exactly as it does in fact. Thus we arrive at our twelfth corollary:

XII. *Organisms will learn how to react differentially to a given objective situation according to the drive active at the time, and to react differentially to a given drive according to the objective situation at the time.*

### Summary

The needs of organisms operate both in the formation of habits and in their subsequent functioning, i.e., in primary motivation. Because of the sensitizing or energizing action of needs in this latter rôle, they are called *drives*.

A great mass of significant empirical evidence concerning primary motivation has become available within recent years. A survey of this material, particularly as

related to hunger, thirst, injury (including the action of very intense stimuli of all kinds), sex, and the action of certain substances such as caffeine, has led to the tentative conclusion that all primary drives produce their effects by the action of various chemicals in the blood. Substances like caffeine, through bathing the neural mechanisms involved, seem to operate by heightening the reaction potential mediated by all positive habit tendencies. Drive substances, such as the various endocrine secretions, are conceived either to be released into the blood by certain kinds of strong stimulation or as themselves initiating stimulation of resident receptors through their evocation of action by selected portions of the body, e.g., the intestinal tract and the genitalia. In both cases the energy effecting this receptor activation is called the drive stimulus ( $S_D$ ).

The action of these endocrine substances, while apparently lowering the reaction threshold of certain restricted effectors (1, p. 184 ff.), seems also to have a generalized but possibly weaker tendency to facilitate action of *all* effectors, giving rise to a degree of undifferentiated motivation analogous to the Freudian libido. Thus a sex hormone would tend to motivate action based on any habit, however remote the action from that involved in actual copulation. This, together with the assumption that one or more other motivations are active to some degree, explains the continued but limited amount of habitual action of organisms when the motivation on the basis of which the habit was originally set up has presumably become zero. It also suggests a possible mechanism underlying the Freudian concept of sublimation. However, where differential behavior is required to bring about reduction in two or more drives, the differences in the drive stimuli characteristic of the motivations in question, through the principle of afferent interaction and the resulting stimulus patterning, suffice to mediate the necessary discrimination.

The hypothesis of the endocrine or chemical motivational mechanism and the associated principle of the drive stimulus, when coupled with various other postulates of the present system such as primary reinforcement, primary stimulus generalization, and the opposi-

tion of experimental extinction to excitatory potential, seem to be able to mediate the deduction, and so the explanation, of nearly all the major known phenomena of primary motivation.<sup>4</sup> In addition to the phenomena already summarized there may be mentioned the further deductions flowing from the system: that resistance to extinction maintains a consistent growth function of the number of reinforcements for any constant drive; that the asymptotes of these growth functions are themselves functions of the strength of drive; that for constant habit strengths, reaction potential has a positive acceleration for increasing drives between zero and the drive employed in the original reinforcement; that if habit strength is zero, reaction tendency is zero; that an increase in drive will over-ride the total extinction of a reaction potential arising from a weaker drive; that in a given objective habit situation the abrupt shift from one drive to another will, in the absence of discriminatory training, disrupt the behavior to some extent, though not completely; that transfers of training (habits) from one motivation to another will be prompt and extensive; that organisms in the same external situations will learn to react differentially in such a way as to reduce different needs; that the conditioned evocation of endocrine secretions facilitates the evocation of muscular activity on the subsequent presentation of appropriate conditioned stimuli, which is believed to be the rôle of "emotion" in the motivation of behavior.

On the basis of the various background considerations elaborated at the preceding pages, we formulate our sixth and seventh primary molar laws of behavior:

### *Postulate 6*

Associated with every drive ( $D$ ) is a characteristic drive stimulus ( $S_D$ ) whose intensity is an increasing monotonic function of the drive in question.

<sup>4</sup> One class of phenomena seems to involve the action of fractional antedating goal reactions and of spatial orientation. Space is not here available for the elaboration of these mechanisms and their action in motivational situations.

*Postulate 7*

Any effective habit strength ( ${}_s\bar{H}_R$ ) is sensitized into reaction potentiality ( ${}_sE_R$ ) by all primary drives active within an organism at a given time, the magnitude of this potentiality being a product obtained by multiplying an increasing function of  ${}_sH_R$  by an increasing function of  $D$ .

From Postulates 5, 6, and 7 there may be derived the following corollary:

*Major Corollary II*

The amount of reaction potentiality ( ${}_sE_R$ ) in any given primary motivational situation is the product of (1) the effective habit strength ( ${}_s\bar{H}_R$ ) under the existing conditions of primary drive multiplied by (2) the quotient obtained from dividing the sum of the dominant value of the primary drive ( $D$ ) plus the aggregate strength of all the non-dominant primary drives ( $\bar{D}$ ) active at the time, by the sum of the same non-dominant drives plus the physiological drive maximum ( $M_D$ ).

*References*

1. BEACH, F. A. Arousal, maintenance, and manifestation of sexual excitement in male animals. *Psychosomatic Medicine*, 1942, 4, 173-198.
2. ELLIOTT, M. H. The effect of change of drive on maze performance. *Calif. Pub. in Psychol.*, 1929, 4, 185-188.
3. FINCH, G. Hunger as a determinant of conditional and unconditional salivary response magnitude. *Amer. J. Physiol.*, 1938, 123, 379-382.
4. HEATHERS, G. L., and ARAKELIAN, P. The relation between strength of drive and rate of extinction of a bar-pressing reaction in the rat. *J. Gen. Psychol.*, 1941, 24, 243-258.
5. HOVLAND, C. I. The generalization of conditioned responses: II. The sensory generalization of conditioned responses with varying intensities of tone. *J. Genet. Psychol.*, 1937, 51, 279-291.
6. HULL, C. L. Differential habituation to internal stimuli in the albino rat. *J. Comp. Psychol.*, 1933, 16, 255-273.

7. HULL, C. L. The rat's speed-of-locomotion gradient in the approach to food. *J. Comp. Psychol.*, 1934, 17, 393-422.
8. LEEPER, R. The role of motivation in learning: A study of the phenomenon of differential motivational control of the utilization of habits. *J. Genet. Psychol.*, 1935, 46, 3-40.
9. MILLER, N. E., and MILES, W. R. Effect of caffeine on the running speed of hungry, satiated, and frustrated rats. *J. Comp. Psychol.*, 1935, 20, 397-412.
10. MOORE, C. R., PRICE, D., and GALLAGHER, T. F. Rat prostate cystology and testes-hormone indicator and the prevention of castration changes by testes extract injection. *Amer. J. Anat.*, 1930, 45, 71-108.
11. PAVLOV, I. P. Conditioned Reflexes (trans. by G. V. Anrep). London: Oxford Univ. Press, 1927.
12. PERIN, C. T. Behavior potentiality as a joint function of the amount of training and the degree of hunger at the time of extinction. *J. Exper. Psychol.*, 1942, 30, 93-113.
13. SKINNER, B. F., *The Behavior of Organisms*. New York: D. Appleton-Century Co., Inc., 1938.
14. SKINNER, B. F., and HERON, W. T. Effect of caffeine and benzedrine upon conditioning and extinction. *Psychol. Record*, 1937, 1, 340-346.
15. STONE, C. P. The retention of copulatory ability in male rats after castration. *J. Comp. Psychol.*, 1927, 7, 369-387.
16. STONE, C. P. The retention of copulatory activity in male rabbits following castration. *J. Genet. Psychol.*, 1932, 40, 296-305.
17. STONE, C. P. Activation of impotent male rats by injections of testosterone propionate. *J. Comp. Psychol.*, 1938, 25, 445-450.
18. SWITZER, S. A. The effect of caffeine on experimental extinction of conditioned reactions. *J. Gen. Psychol.*, 1935, 12, 78-94.
19. WARDEN, C. J., JENKINS, T. N., and WARNER, L. H. *Introduction to Comparative Psychology*. New York: Ronald, 1934.
20. WILLIAMS, S. B. Resistance to extinction as a function of the number of reinforcements. *J. Exper. Psychol.*, 1938, 23, 506-521.
21. YOUNG, P. T. *Motivation of Behavior*. New York: John Wiley and Sons, 1936.
22. ZENER, K. E., and MCCURDY, H. G. Analysis of motivational factors in conditioned behavior: I. The differential effect of changes in hunger upon conditioned, unconditioned, and spontaneous salivary secretion. *J. Psychol.*, 1939, 8, 321-350.

## Four Fundamentals of Learning

N. E. MILLER AND J. DOLLARD

*The four fundamentals alluded to in the title are drive, cue, response and reward. The actions and interactions of these concepts comprise the motivation theory of Miller and Dollard. Miller was a student of Hull and their theories have much in common.*

### DRIVE

A drive is a strong stimulus which impels action. Any stimulus can become a drive if it is made strong enough. The stronger the stimulus, the more drive function it possesses. The faint murmur of distant music has but little primary drive function; the infernal blare of the neighbor's radio has considerably more.

While any stimulus may become strong enough to act as a drive, certain special classes of stimuli seem to be the primary basis for the greater proportion of motivation. These might be called the primary or innate drives. One of these is pain. Pain can reach stabbing heights of greater strength than probably any other single drive. The parching sensation of thirst, the pangs of extreme hunger, and the sore weight of fatigue are other examples of powerful innate drives. The bitter sting of cold and the insistent goading of sex are further examples.<sup>1</sup>

---

Excerpted from N. E. Miller and J. Dollard, *Social Learning and Imitation*. Published for the Institute of Human Relations by the Yale University Press, 1941.

<sup>1</sup> *Technical note:* Where subjective terms are used, they were adopted in order to express the main point in briefer phrases more meaningful to many readers and are not meant to have any refined technical or philosophical significance. Thus the specialist will understand that thirst can be defined objectively

To people living in a society protected by a technology as efficient as ours, it is difficult to realize the full height to which these primary drives can mount. One of the basic aims of any social organization is to protect its members from the unpleasant force of severe motivation by providing satiation for drives before they mount to agonizing heights. Thus it is only when the social organization breaks down under extreme conditions of war, famine, and revolution that the full strength of the primary drives is realized by the social scientist in his usually secure social circumstances.

The importance of the innate drives is further obscured by social inhibitions. In those cases in which our society allows a primary drive, for example the sex drive before marriage, to rise to considerable heights, a certain amount of negative sanction or social opprobrium generally attaches to frank statements about the drive and to vivid descriptions of its intensity. In some cases, the effects of this taboo upon speech spread even to thoughts, so that consciousness of the drive tends to be weakened and, in extreme cases, obliterated.

The conditions of society tend, besides obscuring the rôle of certain primary drives, to emphasize certain secondary or acquired drives. These secondary drives are acquired on the basis of the primary drives, represent elaborations of them, and serve as a façade behind which the functions of the underlying innate drives are hidden. Such experimental evidence as is available indi-

---

in terms of the effects of dehydration. The fact that animals can be trained to turn to the right when they have been deprived of water and to the left when they have been deprived of food (Leeper, 1935) is the objective basis for stating that thirst is a cue. The fact that a certain amount of dehydration increases the general level of activity is the objective basis for stating that thirst is a strong stimulus. This statement is further confirmed by the fact that the drinking, which reduces the increased level of activity, also has a rewarding effect. In further examples, it will be understood that hearing a sound of a given pitch means being stimulated by sound waves of a given frequency; feeling oneself run means stimulating certain proprioceptors in the course of making the responses involved in running; and trying a response means that the response is elicited.

cates that the acquired drives cannot remain indefinitely independent, but tend, like any habit, to become gradually weaker unless occasionally rewarded. A possible mechanism for the production and maintenance of acquired drive will be discussed briefly in the fourth chapter.

One of the strongest of the acquired drives is anxiety or fear. This drive mirrors pain, as it were, and is probably based primarily upon it. Various appetites are similarly based on other innate drives, such as hunger, thirst, and sex. Appetites can often summate with primary drives and provide a considerable portion of the motivation in circumstances under which the drive would otherwise be quite weak. Such acquired drives or appetites vary according to the social conditions under which they are learned and often impart a cultural coloring to the innate drives. Thus hunger may take the form of an appetite for particular foods; sex, that of an attraction to beautiful women; thirst, that of a desire for a specific type of drink. In short, acquired drives may assume the aspect of social needs.

Some of the stronger of the acquired drives or social needs are not based on any single drive, but rather on a number of more primary drives. Indeed, it is probably from this fact that they derive their strength and persistence. Thus, the desire for money is the focus of many needs. During the course of his socialization, the individual learns that the possession of money is the means of gratifying many different needs and that the lack of money is a signal that he may have to bear the uncomfortable goading of many unsatisfied desires. Some of the drives upon which the desire for money is based are primary drives, such as hunger and cold. Others may be secondary, such as anxiety. Because of the number of different drives supporting the need for money, it is a rare occasion when the individual is without any primary motivation to summate with and activate his need for money. In fact, so omnipresent in our adult society is the desire for certain coins and bits of paper that it seems almost to operate as a primary drive. Yet it is obvious that this desire is not innate. Children have to be taught the value of money.

What may be called the desire for approval is established earlier than the craving for money. Like money, approval has been repeatedly associated with the gratification of many different primary drives, while disapproval has been repeatedly associated with the mounting of pain, hunger, and other forms of drive discomfort.

Such terms as pride, ambition, and rivalry point to another powerful core of acquired drives. These are probably related to the desire for approval, but are somewhat more generalized and have crystalized into the desire for institutionalized symbols of approval somewhat analogous in function to money. These trappings of prestige arouse responses of deference from others. They may, in fact, be more widely useful than money itself. In our society, for example, it seems to be easier to use the great prestige of a position at the top of the social hierarchy as an instrument to obtain money than it does to buy with money a position among the "best families."

Differences between one's own behavior and dress and those of one's associates can become cues arousing acquired motivation based on pain and other innate drives. Thus, under appropriate conditions, an acquired drive to match or imitate one's associates, particularly those enjoying superior prestige, can be established. The special conditions for establishing an acquired drive to imitate will subsequently be described in detail.

The interlacing network of acquired drives appears to be based upon primary drives and rewards. It is one of the weaknesses of social science that neither the sociological conditions nor the psychological mechanisms for the acquisition of these drives have as yet been satisfactorily determined.

Without drives, either primary or acquired, the organism does not behave and hence does not learn. A dramatic example of the importance of drive may be drawn from the literature of experimental psychology. It was once thought that old rats are more stupid than young rats. Certainly they learned more slowly in the laboratory experiments which had been conducted up to that time. Finally it occurred to an experimenter,

Stone (1929a, p. 104),<sup>2</sup> that mature animals kept underweight may not be starved as much as young growing animals, even though the latter are allowed to gain. When he took especial care to see to it that both the old and the young rats were motivated by a maximal hunger drive, the apparent stupidity of the old rats disappeared.

Any teacher who has tried to teach unmotivated students is aware of the relationship between drive and learning. Completely self-satisfied people are poor learners. Colonial governments have sometimes found it necessary to tax satisfied natives in order to create a need for money. Spurred by the prospect of interference with the satisfaction of their more primary drives, the natives would then learn the new work and continue to perform it for money.

## CUE

The drive impels a person to respond. Cues determine when he will respond, where he will respond, and which response he will make.<sup>3</sup> Simple examples of stimuli which function primarily as cues are the five o'clock whistle determining when the tired worker will stop, the restaurant sign determining where the hungry man will go, and the traffic light determining whether the driver will step on the brake or on the accelerator.

The relationship between the drive and cue functions of stimuli must be considered in more detail. Stimuli may vary in two respects: in strength and in kind. Thus sounds may differ in loudness and in pitch. Weak sounds have little innate drive value; the naïve child is not stirred to action by the onset of a cricket's chirp nor rewarded by its cessation. But weak sounds may be distinctive and have cue value; the individual can be trained to make one response to a weak sound of high

<sup>2</sup> C. P. Stone, 1929a, the Age Factor in Animal Learning: I. Rats in the problem box and in the maze. *Genetic Psychology Monographs*, 5, 1-130.

<sup>3</sup> This way of describing cues was suggested to the authors in conversation by Dr. John W. M. Whiting.

pitch and a different response to a weak sound of low pitch. The more the two sounds differ in pitch, the easier it is to connect different responses to them; they are more *distinctive* as cues.

As sounds become louder, they increase in drive value. A noise of medium intensity may have a dynamogenic effect, causing the individual to be slightly more active and less likely to fall asleep. As sounds become extremely loud, they possess definite innate drive value and can arouse even the naive infant to action. Furthermore, escape from an exceedingly loud noise acts as a reward. But loud sounds may vary in pitch and hence can serve as cues as well as drives. The individual can be trained to make one response to one loud sound and a different response to another.

Finally, different strengths of stimulation may themselves be distinctive and hence serve as cues. A child can learn to respond in one way to a word spoken softly and in a different way to the same word spoken loudly.

In general, stimuli may vary quantitatively and qualitatively; any stimulus may be thought of as having a certain drive value, depending on its strength, and a certain cue value, depending on its distinctiveness.

The occurrence of a specific response can be made to depend not upon any single stimulus alone, but upon a pattern of stimuli. A hurried driver speeding down the highway may respond differently to the combination of a sign indicating a reduced speed limit and a police car seen in the rear-vision mirror than he would to either the sign without the police car or the car without the sign.

Since drive and cue functions are two different aspects of the same thing, a stimulus, any given stimulus may possess, like the loud sound, an important amount of both functions. Thus a drive stimulus such as hunger may have a selective or cue function as well as its impelling or drive function. In fact, hunger is a part of the stimulus pattern involved in determining the response to the restaurant sign. An individual standing between a restaurant sign to the right and a hotel sign to the left will respond to the stimulus pattern of drive

plus signs by turning to the right if he is hungry and to the left if he is tired.

Since many drives, such as hunger and fatigue, originate within the individual and are thus present wherever he goes, they are usually not specific enough, without support from more distinctive cue-stimuli, to elicit the correct responses. Hunger as the sole cue is not highly correlated with reward for the response of biting. Other cues must be present to indicate whether or not the thing bitten will be palatable.

The importance of cues in the learning process becomes apparent from an examination of cases in which learning fails through the absence of cues. The experiment already described of hiding candy under a certain book was repeated on a four-year-old child. When the candy was hidden under a distinctive red book in the middle of a row of black books, he learned to respond perfectly by the third trial. When the candy was hidden under a dark book in the middle of a long row of books of similar color, he learned during the first few trials to select books in that general region, but thereafter failed to show improvement during the next ten trials. If the cues are too obscure, as in this case, it is impossible to learn to make the correct response with precision.

Noticing a cue can in itself be a response which may be learned. This is called learning to pay attention.

## RESPONSE

Drive impels the individual to respond to certain cues. Before any given response to a specific cue can be rewarded and learned, this response must occur. A good part of the trick of animal training, clinical therapy, and school teaching is to arrange the situation so that the learner will somehow make the first correct response. A bashful boy at his first dance cannot begin to learn either that girls will not bite him or how to make the correct dance step until he begins responding by trying to dance.

The rôle of response in human learning is sometimes rather difficult to observe, because of the fact that when

the individual already has a good deal of social learning behind him, verbal and other nonovert anticipatory responses may play an important part in controlling his behavior. But cases of verbal behavior are no exception to the rule. A person cannot learn a new way of speaking or thinking until he has first tried a new statement or thought. Much of the difficulty in teaching arises in finding a situation which will produce thoughts that can be rewarded.

The case with which a response can be learned in a certain situation depends upon the probability that the cues present can be made to elicit that response. It is a case of "unto him who hath shall be given." If the response occurs relatively frequently, it is easy to reward that response and still further increase its frequency of occurrence. If the response occurs only rarely, it is difficult to find an occasion when it occurs and can be rewarded. Thus, the initial tendency for a stimulus situation to evoke a response is an important factor in learning. In order to describe this factor, one may arrange the responses in the order of their probability of occurrence and call this the *initial hierarchy* of responses. The most likely response to occur is called the dominant response in the initial hierarchy; the response least likely to occur is called the weakest response. The same situation may be described in another way. It may be said that there is a strong connection between the stimulus and the dominant response and a weak connection between the stimulus and the weakest response. The word "connection" is used to refer to a causal sequence, the details of which are practically unknown, rather than to specific neural strands.

Learning changes the order of the responses in the hierarchy. The rewarded response, though it may have been initially weak, now occupies the dominant position. The new hierarchy produced by learning may be called the *resultant hierarchy*. At the beginning of the experiment with the books, the response of selecting the correct book happened to be a late response in the initial hierarchy, coming after thirty-six responses of looking under other books and after other responses such as asking questions. At the end of the experiment,

this response had become the first or dominant response in the resultant hierarchy.

Usually the order of responses in an initial hierarchy is the result of previous learning in similar situations. In those cases in which the order of the response is primarily determined, not by learning, but by hereditary factors, the initial hierarchy may be called an *innate hierarchy*. In the human infant, crying occupies a higher position in the innate hierarchy than does saying the word "No." Therefore it is much easier for an infant to learn to respond to the sight of a spoonful of medicine by crying than by saying "No."

Once having learned, the person responds in a new way. But if the correct response must always occur before it can be rewarded, what novelty is added by learning? The new feature is that the particular response rewarded now occurs regularly to a specific cue, whereas previously its occurrence at just that time and place may have been exceedingly infrequent. The connection between cue and response is the new product of learning. Often a number of different response units are connected to cues so that they all occur together, either simultaneously or successively. Thus a new pattern of responses is produced; the responses are old, but the combination is new. Once this new combination occurs frequently, variations in it may be points of departure for still further learning.

There are a number of different ways in which the response to be connected to a given cue as a new habit may first be elicited. The least efficient of these is the mechanism of random behavior. The drive elicits responses, one after another. As those high in the initial hierarchy are non-rewarded and extinguished, various weaker responses appear. These are new in the sense that they would not have been likely to occur before the stronger competing responses were extinguished. If one of these happens to be the desired response, it can be rewarded and the habit established.

After a person has learned to attach appropriate responses to specific words as cues, language can be of enormous assistance in eliciting the correct response early in the sequence. The problem confronting the

little girl was vastly simplified by eliminating many irrelevant responses through the simple expedient of telling her that the candy was under a book in the bookcase.

Similarly, after a person has learned to attach appropriate responses to the cue of seeing another person perform an act, imitation can help the person to limit his range of trial and error. Provided he has learned the particular units essential to successful copying (a complex process which will be described in detail later), watching a demonstration of the correct response may enable the student to perform perfectly on the first trial. But if not all of the units and the techniques of combining them have been learned, the first trial may be halting and involve errors.

One important function of culture, as Ford (1937; 1939)<sup>4</sup> has pointed out, is that it represents a storehouse of solutions to recurrent problems. Various means of instruction are employed by the older members of society to get the younger members to perform just those responses which are most likely to be rewarded.

In psychological literature, insight and conditioning are often thought to be at the opposite poles, yet both are mechanisms which can cause the individual to perform the correct response on the first trial. As an example of conditioning, a child may thrust his hand against a hot radiator, be burned, and learn to withdraw it at the sight of the radiator. The question here is: How does the child come eventually to withdraw his hand from the radiator before he is burned? Obviously it is the primary or unconditioned stimulus of the burn which causes the child immediately to try withdrawal—the dominant response in his innate hierarchy of responses to this situation. This withdrawal is rewarded by escape from pain. Thus the first trial is a successful one; there are no errors. But at the same time that the child is withdrawing from pain, he is also withdrawing from the sight of the radiator and is rewarded for doing

C. S. Ford, A Sample Comparative Analysis of Material Culture. From G. P. Murdock (Ed.), *Studies in The Science of Society*. New Haven: Yale University Press, 1937, 225-246.

C. S. Ford, Society, culture, and the human organism. *J. Gen. Psychol.*, 1939, 20, 135-179.

this. Thus a tendency is established for visual cues to elicit withdrawal before his hand touches the radiator. The important fact about conditioning, or associative learning, is that the correct response is dominant in the hierarchy of responses to the unconditioned stimulus. Therefore, that stimulus causes the subject to make the correct response on the first trial.

In cases involving insight or reasoning, a more complicated and less understood mechanism may achieve the same result. In a new situation, the function of reasoning or insight seems to be to produce a response which might otherwise not have been made. If this response is rewarded, it will be learned as the response to that situation. If the insight is not rewarded, it will be abandoned. The authors use all the insight at their command, facilitating their reasoning by the use of verbal and even mathematical symbols, in attempting to prepare demonstrations for their classes. If these demonstrations are successful, they are repeated year after year; if not, they tend to be abandoned. Insight, the conditioning technique, imitation, and verbal instruction are different ways of producing responses likely to be rewarded. Once produced, the responses are all subject to the same laws of trial-and-error learning, to rejection or selection on the basis of the effects of non-reward or reward.

If a response which would be rewarded does not occur, it is not learned. Radically new inventions are rare because the occurrence of the correct combination of responses is improbable. No one in the pre-Columbian New World ever made a wheel, even though the Mayas built huge pyramids and the Incas had paved roads. In all the rest of the world, the wheel seems to have been invented only once. The history of human society, is teeming with the unborn spirits of useful responses which for centuries did not occur, were therefore not rewarded, and did not become cultural habits.

## REWARD

Drive impels the person to make responses to cues in the stimulus situation. Whether these responses will

be repeated depends on whether or not they are rewarded. If the response is non-rewarded, the tendency to repeat it to the same cues is weakened. Pavlov (1927)<sup>5</sup> has called this process *extinction*; it will be discussed in more detail in the next chapter. In cases of sophisticated human learning, the process of extinction is facilitated by the learned habit of abandoning unsuccessful responses quickly. Thus in the experiment, the little girl went back to look under the same book only a few times and only on the first trial. The failure to see candy became a cue to abandon looking under that book.

As the dominant response is weakened by non-reward, the next response in the hierarchy becomes dominant. As successive responses are eliminated by non-reward, the individual exhibits variable or what has perhaps been misnamed *random behavior*. It is this variability that may lead to the production of a response which will be rewarded.

If one of the so-called random responses is followed by an event producing a reduction in the drive, the tendency to make this response on subsequent exposure to the same cues is increased. In other words, the connection is strengthened between the stimulus pattern (drive and other cues) and the response. Events producing such strengthening are called rewards. A more technical name for reward is *reinforcement*. Relief from pain is a reward. Drinking water when thirsty, eating food when hungry, and relaxing when tired are other examples of primary, or innate, rewards.

If rewards are thought of as events producing a reduction in the drive, that is, in strength of stimulus, the relationship between satiation and reward becomes clear. Since it is impossible further to reduce the strength of a drive stimulus which is already zero, reward is impossible in the absence of drive. Thus, gulping food is no reward to a satiated animal and may even become painful so that regurgitation is rewarding.

If rewards produce reductions in drive, then too rapid repetition inevitably leads to satiation of the drive, with

<sup>5</sup> I. P. Pavlov, *Conditioned Reflexes*. London: Humphrey Milford, Oxford University Press, 1927.

the result that the rewards lose their rewarding value until the drive reappears. In the absence of reward, the acts which have led to a previously rewarding event tend to be weakened through extinction. Such weakening is one of the factors which eventually cause responses appropriate to a given drive to cease in the absence of that drive. Did rewards not tend to weaken drives, there would be no mechanism for causing the individual to stop one line of satisfying behavior and turn to another.

Though it is convenient to think of rewards as events producing reductions in the strength of the drive stimulus, it is not necessary to be able to identify the drive which is reduced and the manner in which it is reduced in order to be able to determine empirically that certain events are rewards under certain circumstances and to make practical use of this information. Once it has been discovered that a given event, such as receiving praise from the mother, can be used as a reward to strengthen a given stimulus-response connection, e.g., the connection between the cue of a drippy feeling in the nose and the response of blowing the nose, it can be assumed that this same event can be used as a reward to strengthen other stimulus-response connections. Here the drive is probably some form of anxiety or desire to please the mother, but it is not absolutely essential to be able to identify the drive in order to discover that praise is a reward.

Any event known to strengthen stimulus-response connections may be called a reward. The term "reward" will be used hereinafter to refer to drive reduction, to events (such as eating when hungry) from which drive reduction may be reliably predicted, to the object (such as food) producing the drive reduction, and to other events empirically found to have the effect of strengthening cue-response connections. In the last instance, the definition is not circular so long as the fact that the event was found to strengthen one connection is used to predict that it will strengthen others.

The mention of praise brings up the topic of acquired rewards or, as they may sometimes be called, *secondary rewards*. Just as it is possible for previously neutral stimu-

lus situations to acquire drive value, so also is it possible for previously neutral stimulus situations to acquire reward value. Relief from anxiety is an acquired reward. Receiving money, social approval, and higher status are other events with acquired reward value. Acquired rewards are enormously important in social life.

Experimental evidence indicates that the acquired reward value of an object or event is based upon more primary, innate rewards. This is confirmed by cross-cultural data. In different cultures, different objects and events have widely differing degrees of acquired reward value. In our society, metal and paper are used for money, among the Kwoma, shells. More interesting is the fact that in our society money is the route to many primary rewards and is exceedingly important as a secondary reward, while among the Kwoma money is primarily of use only to maintain friendly (i.e., non-aggressive) kinship relations and is less important as a secondary reward.<sup>6</sup> Experimental evidence indicates that the acquired reward value of an object or event tends to become weakened, like any other learned response, unless it is itself supported occasionally by some innate reward. An inflation provides conditions under which money loses value, but the dependence of some of the other acquired rewards upon more primary rewards is difficult to trace through the intricate fabric of our society. The mechanism of acquired rewards will be discussed in somewhat more detail later.

The same object may serve as an incentive with acquired drive value in one situation and as a reward in another. Food at a tantalizing distance may serve as an incentive to more vigorous responses, and food in the mouth as a reward strengthening those responses. Similarly, being promised money is an incentive and receiving money, a reward. The incentive value of the object is based upon the more primary reward function. Food at a tantalizing distance loses its incentive value if one is never able to eat it, and the promise of money loses

\* These data on the Kwoma were called to the attention of the authors by Dr. John W. M. Whiting. See his *Becoming a Kwoma* (1941).

its incentive value if one is never paid. Common speech uses reward in a broad sense to include incentives; we shall attempt to use it in the narrower sense of its primary function of directly strengthening cue-response connections.

A connection strengthened by a reward functions automatically; seeing a child run out in front of the car will cause an experienced driver, riding as a passenger in the front seat, to press on the floorboard. The response has been rewarded by escape from anxiety when his own car has stopped in the past; when the passenger sees the child, he now presses his foot down without pausing to think: "I should stop the car, and this will do it." Indeed, as soon as he has time to think, he knows that the response is useless in this situation.

Often in human behavior, the causal sequence is more complex. A thirsty person in a strange house may turn on the water in the faucet to the right and find that it gets warmer instead of colder. Because of past experience with similar situations, he is likely to say to himself, "Oh, in this house the faucet on the left must be the cold water," at the same time turning on the water in the left-hand faucet. As the water gets colder, he may say to himself, "It's funny that the faucet on the left is the cold water." Soon the water is cold enough. He drinks it, and his series of acts is capped with a reward. The next time he comes into the same kitchen, he will have a greater tendency to reach to the left than before, although it may not be strong enough to overcome the strong habit of reaching to the right. While reaching to the right, he may suddenly react with one of the verbal responses which he made and was rewarded for in the previous situation. As soon as he says, "The faucet on the left is the cold water," this operates as both an incentive and a cue to elicit the response of reaching to the left. In this case a statement, or thought, about the reward (cold water) has been an important determinant of his behavior; stated in nontechnical terms, the man turns to the faucet on the left after realizing that it contains the cold water.<sup>7</sup>

<sup>7</sup> Such behavior may correctly be said to involve *expectancy*.

The mechanism described above is common in adult human behavior. Since statements or thoughts about rewards frequently occur in the course of overt behavior, there may be a tendency to think that rewards function primarily in this way and to neglect the more fundamental automatic strengthening of a connection upon which even this mechanism is based. That the strengthening of connections really is more fundamental becomes obvious as soon as the question arises: How do the external cues happen to elicit the thought of the reward as an internal stimulus, and how is this internal stimulus connected with the overt response? Such connections seem to follow exactly the same laws as any others: they are strengthened by reward and extinguished by non-reward. The individual has had to learn as a child to respond appropriately, first when someone else tells him that the reward is to the left, and then when he tells himself that it is there. He has also had to learn to say to himself, in this particular situation, that the cold water is on the left. If the water had not appeared as a reward, he would not have been so likely to respond again with the same statement. Even in cases in which the individual is responding to the "hope of reward," this is founded on *past* reward. If cold water actually does appear, it makes the whole response series more probable the *next* time.

Without reward, people fail to learn. For example, a certain boy never learned much about playing a piano because his mother failed to understand the importance of praise and escape from anxiety as necessary rewards in this situation. She sat at his side during practice. As soon as he finally hit the right note, she would say, "Now, can't you get the fingering correct?" As soon as he got the fingering correct, she would say, "Now, can't you play a little faster?" As soon as he played a little faster, she would say, "But the expression is very poor." Thus no response except that of walking away from the piano at the end of the session was followed by a complete escape from the anxiety-provoking criticism. The boy was never allowed to bask in the relaxing sense of achievement and reduced anxiety that comes with praise or the cessation of criticism. Under these condi-

tions, the one response which was significantly rewarded, to wit, walking away from the piano, became more and more dominant in the situation. The boy failed to learn and, as soon as he was able, escaped practicing altogether.

Part of the seeming mental inferiority of lower-class children at school may be traced to lack of reward. In the first place, the teachers are not so likely to pay attention to them, praise them, and confer little signs of status, such as special tasks, as they are to reward middle-class children in these ways. In the second place, these children have never experienced, or seen at close hand in the lives of relatives, those advantages of better jobs which are the rewards for educational merit, and they consequently see less promise of attaining such positions. The teacher is less likely to reward them, and their own training has invested the types of event which the teacher controls with less acquired reward value.<sup>8</sup>

As long as an individual is being rewarded for what he is doing, he will learn these particular responses more thoroughly, but he may not learn anything new by trial and error.<sup>9</sup> This is partly because the further strengthening of the dominant responses makes the occurrence of any new responses less likely, and partly because its rewards, if ample, will keep the drive at a low level. Thus, in order to get the individual to try a new response which it is desired that he learn, it is often necessary to place him in a situation where his old responses will not be rewarded. Such a situation may be called a *learning dilemma*. In the absence of a dilemma, new learning of the trial-and-error type does not occur. For example, a mother was worried because her child seemed to be retarded in learning to talk. Brief questioning revealed that she was adept at understanding the child's every want as expressed by its gestures. Having other successful means of responding, the child was not in a dilemma. He only learned his old habits of using ges-

<sup>8</sup> For a more detailed description of these conditions, see Davis and Dollard (1940), Chap. XIII.

<sup>9</sup> His behavior may change through other mechanisms, however, such as that of the *anticipatory response*.

tures more thoroughly and consequently did not perform that type of random vocal behavior which would lead to speech. By gradually pretending to become more stupid at understanding gestures, the mother put the child in a dilemma and probably facilitated the learning of speech. At least, under these modified conditions, this child rapidly learned to talk.

The absence of a dilemma is one of the reasons why it is often difficult to teach successful people new things. Old, heavily rewarded habits must be interrupted before new learning can occur. When the accustomed rewards are withdrawn by unusual circumstances such as revolution, new responses may occur and, if rewarded, may be learned; Russian counts *can* learn to drive taxicabs and countesses to become cooks.

Examples of seemingly incidental learning may appear to contradict the assertion that drive and reward are essential conditions of learning. It seems probable, however, that such examples are merely instances of faulty analysis of obscure conditions of acquired drive and acquired reward. An individual whose life history is rather well known to the authors shows almost no incidental learning during many trips from New Haven to New York in the cars of various friends. That he is not mentally deficient is demonstrated by the fact that he shows a good deal of incidental learning when walking through strange forests. He has never been punished for failing to note directions while driving with intelligent friends. But, for failing to note directions carefully enough in the woods, he has on earlier occasions suffered the fatigue of plowing through dense underbrush and crossing steep gullies, has been scratched by thorns, stung by hornets, and gone hungry. Finding the blazes of a trail has been followed by a reduction in the strength of a number of innate drives. As a result of these primary experiences, he now finds that he is slightly anxious whenever disoriented in the woods and that the response of rehearsing landmarks produces a rewarding reduction in this anxiety. Thus, what might be superficially taken

to be an exception actually illustrates the importance of motivation and reward in human learning.<sup>10,11</sup>

<sup>10</sup> Various assumptions of the authors concerning motivation and reward may now be evaluated. It seems almost certain that drives and rewards play an important rôle in the performance of learned responses. This general assumption is an indispensable part of the present exposition of social learning and imitation. Two more specific hypotheses seem quite probable: namely, that drives and rewards are essential to the occurrence of any learning, and that all drive reduction acts as a reward. Finally, and least certainly established, is the hypothesis that reduction in the strength of stimulation is the only source of reward. Because these hypotheses seem to the authors to be the best ones available, an attempt is made to apply them consistently throughout the book. Changes in these hypotheses would alter certain elements throughout the systematic structure of the argument advanced in this book. Most of the practical conclusions are so well founded on empirical fact that major alterations in them seem unlikely. No assumptions are made concerning the mechanism producing the correlation between reward and strength of connection.

<sup>11</sup> *Technical note:* Electrical recording of the responses of afferent nerves indicates that the sudden onset of a new stimulus produces at first a strong burst of impulses from the sense organ which rapidly diminishes in strength till a plateau of stimulation is reached (Adrian [1928], pp. 67, 116). This diminution is called adaptation. According to the principles which have been outlined, such a reduction in strength of stimulation should, if marked enough (as might be the case following the sudden onset of a relatively strong stimulus), act as a reward to any responses associated with it. Careful experiments may reveal that such a mechanism accounts for certain cases of learning which might superficially appear to be exceptions to a rigorous drive-reduction theory of reward.

## A Cognition Motivation Model<sup>1</sup>

EDWARD C. TOLMAN

*University of Chicago*

I shall hold that the development of adequate concepts of motivation and cognition—concepts which will be useful to all the social sciences and not merely to psychology—will be helped by the adoption of a diagrammatic psychological model. The virtues of such a model are, as I see it, two: (1) a diagram, or a set of diagrams, allows the simultaneous presentation of a large number of interrelationships which it would take several pages to state serially in words or equations; and (2) in the working out of such diagrams one is led to conceive of types of interrelationship which may be very important and which yet would be less obviously thought of, if mere words or equations were used.<sup>2</sup>

---

Reprinted from the *Psychological Review*, 1952, 59, with the permission of the publishers.

<sup>1</sup>This is an abbreviated statement of essentially the same position as that developed in greater detail and presented in a chapter entitled "A Psychological Model" in the joint volume, *Toward a Theory of Social Action*, by Talcott Parsons *et al.*, Harvard University Press, 1951. The writer wishes to express his especial indebtedness to Dr. Leo Postman for help in the development of many of the concepts here presented. This paper, with minor modifications, has been read to graduate students in psychology and/or the other social sciences at Harvard University, the University of Minnesota, Stanford University, Northwestern University, and the University of Chicago.

<sup>2</sup>The paper was put in final form while the writer was Visiting Professor of Psychology at the University of Chicago in the spring quarter of 1951.

<sup>3</sup>The writer wishes to express his thanks to Mr. I. Goldiamond for help in conceiving and drawing the particular diagrams contained in the present paper.

The specific model I shall propose contains three basic constructs which I shall call, respectively: (a) the need system; (b) the belief-value matrix or belief-value matrices; and (c) the immediate, concrete behavior space. Let me begin by talking about (b), the belief-value matrix. Figure 1 presents a small partial matrix. It presents "belief" relationships only.

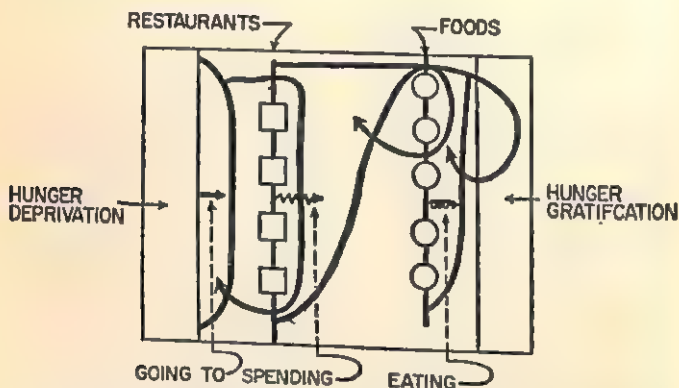


Fig. 1. An individual's, or a group's, belief-value matrix (values omitted) with respect to restaurants and foods.

For the purposes of exposition I am leaving out the accompanying "values" until subsequent figures. The behavior of any mature individual is conceived as being determined in part by a very large number of such small belief-value matrices interlocking, overlapping and including or excluding one another. A very multidimensional diagram would thus be necessary to indicate at one and the same time all these assumed interlocking and overlapping matrices of beliefs (and values).

The cells (squares or circles) in Fig. 1 represent "typed" (i.e., not concrete) "images" of objects or environmental situations, which the individual is conceived to carry around with him as a result of previously learned categorizations and differentiations *re* environmental objects. The cells indicated on any one vertical line represent a set of "similars." And each vertical line

itself represents a "generalization dimension." The particular matrix represented in Fig. 1 contains a generalization dimension *re* restaurant types and another *re* types of objects-to-be-eaten, or foods. The units of these dimensions, if we had them, and the relative spacings along them of the object images would define the degrees of "functional" similarity between such objects—that is to say, the degree of similarity between the different kinds of restaurant relative to the function of leading to foods or between the different kinds of food relative to the function of leading to hunger gratification. They would define these functional similarities as they would operate in helping to determine the choice-behaviors of the particular individual. It is our lack of knowledge concerning all the types of such generalization dimensions, the nature of their units and the spacings of the specific images along them, which is, I believe, one of the reasons why psychology is today so frustrating. We have at present little theoretical or empirical knowledge, save in the cases of simple sensory qualities, as to what the important dimensions and units of generalization are and how the specific subvarieties of objects are spaced in terms of these units. However, a recent important contribution toward the discovery of a new functionally important generalization dimension with which many are familiar lies in the work of Sears and others (2, 3, 6, 7) on doll play. It has turned out that (for the purposes of *aggression*) first the child itself, then the parent doll of the same sex, then the parent doll of the opposite sex, then the child doll of the same sex, then the child doll of the opposite sex, then the baby doll, and finally, imaginary natural forces or "spooks" turn out to be arranged in this order on a single functional dimension. If the child is high in aggression, and if there is little conflicting anxiety, he will himself express the most aggression in his play, make the parent doll of the same sex express the next most, the parent doll of the opposite sex the next most, the child doll of the same sex the next most, the child doll of the opposite sex the next most, the baby doll the next most and imaginary forces such as spooks or winds the least. This, then, is an empirically discovered

new type of generalization—a dimension *re* aggression determinative of the behavior of these children—which is presumably a product of our culture. Children growing up in a culture having a different family constellation from ours might well show a quite different ordering of their dolls for the purposes of aggression. Obviously many other such types of generalization dimensions are what we psychologists would like to find.

Turn now to the looped arrows. These looped arrows (or we might call them lassos) represent what I shall call “beliefs.” These belief lassos are a very important constitutive feature of any matrix. The forked ends at the beginning or left of each arrow, or lasso, represent a generalization spread as regards the type of initial (*terminus a quo*) object or situation. The spread and shape of this forked end of a looped arrow represent the range of types of initial objects or situations which will be accepted, and with what respective readinesses, as appropriate means objects for supporting a given sort of behavior (indicated by the small arrow) in order to get to such and such a further, *terminus ad quem* type of object or situation. And the inclusiveness or spread of the far end of a loop indicates the degree of generalization involved in the belief as to the types of end-object or situation which will be achieved by this given type of behavior. Concretely, this diagram represents a hypothetical case in which an individual (obviously one in our society) has come to *believe* that, if he is hungry (i.e., if he is in the presence of hunger deprivation), he should “go to” such and such types of restaurant and that these restaurants when responded to by “spending” will get him, with such and such respective readinesses, to such and such types of food and that such and such types of food, when responded to by “eating,” will get him finally, with such and such respective degrees of readiness, to hunger gratification.

But this diagram, as given in Fig. 1, presents, as I said, only the cognitive or belief side of the matrix. In actuality such beliefs do not exist apart from their motivational and value accompaniments. Thus the individual will have learned at one and the same time not only that certain foods when eaten lead with such and

such degrees of probability to hunger gratification, but also that such gratification has positive value and that hunger deprivation has negative value, and hence that such and such foods and such and such types of restaurant also have varying degrees of positive value. In other words, a complete diagram of any actually existent matrix must contain not only beliefs but also values—that is, goodnesses or badnesses deposited on the individual cells. In Fig. 2 I have added, therefore, plus and

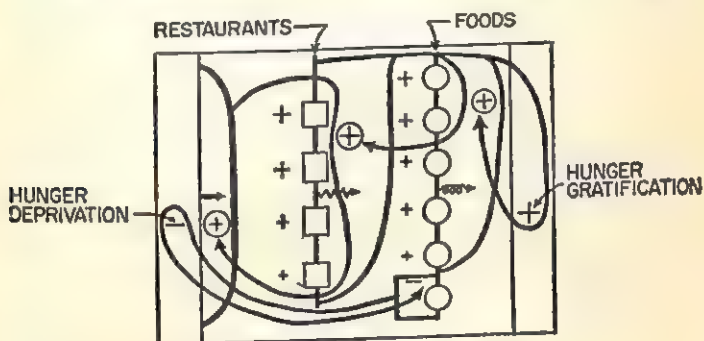


Fig. 2. A belief-value matrix, including values.

minus symbols to represent these values. I have indicated that one type of food is actually believed to lead to hunger deprivation and hence to have negative value.

A further point to be noted about Fig. 2 is that it may be conceived to represent (for a very simple area) a culturally and sociologically determined belief and value system as internalized in the given individual. This belief and value system, this matrix, if correctly inferred by the observer, would be statable in verbal propositions such as:

1) If X (the individual in question) was positively valuing hunger gratification and correspondingly suffering hunger deprivation, he would value such and such types of foods in such and such an order by virtue of his belief concerning their respective properties for producing hunger-gratifications.

2) If X valued positively such and such types of food,

he would be ready to value positively such and such types of restaurants in such and such an order because of his belief that these types of restaurants would lead with such and such degrees of probability to such and such types of foods through the act of spending. He might, of course, also value such types of restaurants negatively because of his beliefs concerning the negative values involved in having to spend money. To represent this latter complication another belief-value matrix *re* spending would have to be added to the diagram.

Further, if questionnaires and verbal reports can be considered reliable forms of data, these propositions about the matrix or matrices could be relatively adequately inferred from such questionnaires or interviews. Thus, for example, we could ask a subject:

1) "What are you ready to do when you haven't eaten for a considerable length of time?"

2) "What kinds of food do you like? Name six varieties of food in order of preference. What do you like about each of these six?"

3) "For each of these six foods what types of restaurant would you go to and in what order? List all the considerations you would take into account in choosing the one kind of restaurant or the other."

The use of such "*attitude questionnaires*," although, of course, far better constructed, or the use of interviews would thus be one general kind of operational technique for getting at the contents of a belief-value matrix. The adoption of such a technique involves, of course, two basic assumptions, neither of which is probably wholly valid. On the one hand, the use of questionnaires or interviews assumes that under the impetus of some other drive such as pure intellectual curiosity, or perhaps merely the desire to please the experimenter, the individual can, so to speak, run over his matrix and report it verbally without producing any serious distortions in the matrix structure itself. And, secondly, the use of these techniques also assumes that the subject's matrix is all verbally available to him—that is, that no essential parts of the matrix are, to use Freudian terminology,

"repressed." Actually, however, we know that important features of many crucial belief-value matrices are in fact unavailable for verbal report. Thus, for example, we may find ourselves, or others, possessing very strong positive or negative values about killing, about war, about Communists, about Democrats, about Republicans, or about signing oaths, in cases in which the underlying beliefs and their generalizations which would explain these values are in large part hidden. Yet the analytical techniques of depth psychology can nevertheless often finally bring such connecting and explanatory beliefs and generalizations to light. Projective techniques can likewise be used to uncover hidden portions of the matrices. However, I would assert that the results of both clinical interviews and projective techniques must in the end be validated against direct observations of resultant actual behavior.

Finally, one further word about matrices. As social scientists, it is to be noted that we may operate with three different levels of matrix. First, we may be interested in a matrix in the sense of a statistically average or modal matrix relative to given types of environmental situations shared by a whole group of similarly placed individuals in a given society or culture. Such a modal matrix, shared by a whole cultural group of individuals and which concerns the most basic and most general features of their environment, is indeed, I believe, what anthropologists have sometimes called the *ethos* of a culture. By conceiving such an *ethos* as a belief-value matrix I am, I think, merely pointing to a more precise and less intuitive way than that usually used by the cultural anthropologists themselves for stating just what such an *ethos* consists of in the way of actual differentiations, generalizations, beliefs and values.

Secondly, there would be the statistically average, modal, matrices which are characteristic of the particular individual. These would state the characteristic, or modal, refinements of differentiation; the characteristic, or modal, spread of generalization; and the characteristic, or modal, beliefs relative to means-end relations and resultant values with respect to given types of goal-objects and given types of means-end activity which

would hold on the average for the given individual. It may be noted in passing that if the particular individual be a very average and very conforming individual, his modal matrices will coincide very closely with the corresponding modal matrices shared by his society as a whole, or at least by most of the other individuals in the same sort of social position in that society that he himself is in.

Finally, thirdly, there would be the activated, determinately valued matrix of the individual on the particular occasion resulting from the precise stimulus situation and the specific drive arousals at that moment. Thus whereas an individual's modal matrix would be defined in terms of the modal magnitudes of his differentiations, generalizations, beliefs and values relative to the given type of activity and the given type of goal, his activated matrix on the particular occasion might, as a result of the particular intensities of his then-and-there aroused drives and of the particular characters and intensities of the stimuli then and there presented to him, contain types and magnitude of differentiation, generalization, belief and value which might be somewhat different from those in his corresponding modal matrix. Thus, for example, if the individual's hunger drive on the given occasion was either excessively strong or excessively weak, the refinements in his differentiations of types of foods might be far less than would be true in the case of a more average magnitude of his hunger drive. Similarly the frequent or emphatic presence in the environment of stimuli corresponding to a particular type of food or a particular type of restaurant might on the given occasion lead to a greater positive value for this food-type or for this restaurant-type than these normally would have.

Turn now to Fig. 3. This diagram indicates that for the final predicting of an actual behavior of a particular individual on a particular occasion we have to know not only the nature of his governing matrix or matrices but also the momentary stimulus and drive conditions to which he is then and there subjected. For these drive and stimulus conditions will determine the then-and-there magnitudes of the individual's needs and hence of

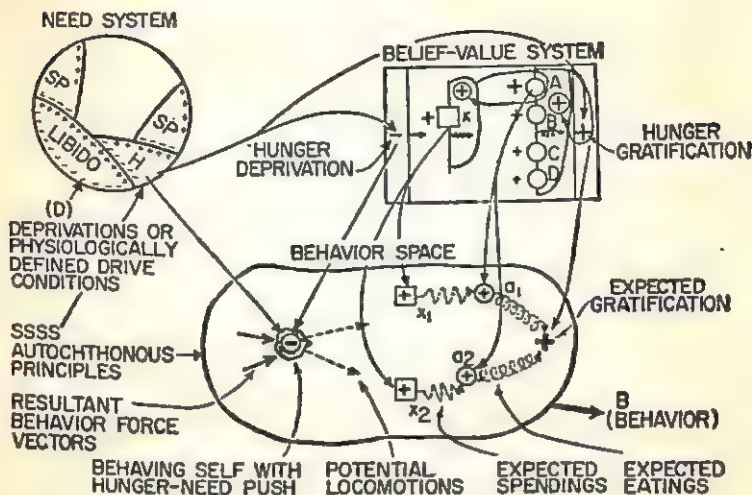


Fig. 3. Independent variables (*D* and *SSSS*); intervening variables (need system, belief-value matrix or system and behavior space); dependent variable (behavior, *B*).

his final goal values. Furthermore, as we have just seen, they may also change slightly the form of his matrix itself—causing it to be more or less differentiated than his corresponding modal matrix. Further, it appears that this activated matrix operates conjointly with the presented stimuli and the autochthonous principles of perception to determine his concrete behavior space, i.e., his perceptual, memorial and expectational processes of the moment, including his need pushes and his valences. It is this behavior space which is conceived to lead, as a consequence of the resulting behavior-force vectors evoked in it, to the “locomotions” of the “Behaving Self.” And it is these locomotions in the behavior space which directly produce the externally observable behavior, *B*.

Lastly, it is to be noted that the resultant behavior may tend to change the external and internal situation for the individual, so that his behavior space, and, ultimately, his matrix, and even his need system, may be different on future occasions. This changing of his be-

havior space and of the matrix may, in the case of simple immediate behavior, consist merely in additive learning, that is to say, in the strengthening of the immediate expectations of the beliefs which were already operative. In other cases involving more restructuring, the changing of the behavior space may lead to the development of wholly new features in the matrix and, finally, such changes and new features in the matrix may lead to the establishment of new or enhanced needs in the Need System.

But let us examine Fig. 3 in more detail. Let us consider further the need system, upper left. This is conceived as a set of interconnected needs. Each need in this set or system, when aroused, is assumed to be a source of both positive and negative values—positive values for need gratification and negative values for continuing need deprivation. These positive and negative values in the needs are indicated by the little plus and minus signs. Different strengths of arousal would be represented by different numbers of such signs. The big need at the left may be thought of as a libido or a source of general energy. This is conceived to be in contact more or less directly with all the specific needs. The specific needs here depicted are *H*, hunger; *Pa*, palatability; and *Sp*, controlled output or "spending." The bifurcating causal arrow from the need system to the two ends of the matrix represents the activation of the positive and negative values of hunger need-gratification and hunger need-deprivation at the moment. The symbol *D* at the left, and underneath the need system, represents the physiologically defined drive conditions which, together with the environmental stimuli, *SSSS*, are conceived to arouse the needs. An aroused need or an aroused set of needs tends to activate any matrix to which it or they are attached. And this activated matrix or these activated matrices act along with the environmental stimuli, *SSSS*, to determine the concrete behavior space which will be evoked. This concrete behavior space consists of the immediately "perceived," "remembered," and hence, "expected" environmental objects plus their positive or negative valences, if they have any, and the immediately perceived and expected "posi-

tion" relations of these environmental objects to one another and to the behaving self. The behaving self is represented also as containing a need-push which comes from the need deprivation in the governing matrix. I have represented the need-push in the present instance as being primarily that coming from hunger. And I have represented the rest of the behavior space as including perceptions of two concrete restaurants  $x_1$  and  $x_2$  with the expected amounts of spending involved in getting to foods  $a_1$  and  $a_2$ , respectively. To sum it up, the behavior space, as drawn, represents the fact that the individual perceives himself as facing two perceived concrete restaurants  $x_1$  and  $x_2$  both of the general type,  $X$ , and each as having a positive valence, and also the fact that he expects to reach such and such concrete instances of foods  $a_1$  and  $a_2$ , each with its positive valence by similar amounts of spending, and finally the fact that he expects these foods to lead on through similar concrete amounts of eating to hunger gratification, which possesses final positive valence. As a result of all this the behaving self experiences or, perhaps better put, "suffers" behavior force vectors, shown by the two little arrows at the left, tending to cause it to "locomote" towards each of the two perceived restaurants.

In Fig. 3, as I have drawn it, I have assumed that the behaving self is faced with a relatively new situation. I have assumed that the particular specific restaurants  $x_1$  and  $x_2$  both of type  $X$  have never been experienced before and that the expectations as to resultant spending, foods, etc., in these two different restaurants are about equal. In other words, I have assumed that the individual's previously established connections with the stimuli are not such as to evoke essentially different perceptions and expectations with respect to the two restaurants. He will expect both restaurants  $x_1$  and  $x_2$  to cost about the same amount. And he will expect both of them to lead to about the same foods  $a_1$  and  $a_2$  both of the same type,  $A$ . They will have equal valences and his behaving self will be pushed by about equal vector forces towards both restaurants. He will in the case diagrammed go towards one or the other restaurant almost by chance. But, later, after having thus behaved

over a number of occasions, he may find out more precisely that one restaurant is better than the other and that they lead to somewhat different foods. That is to say, he may learn. Or, in other words, he may tend to establish new, more precise perceptual discriminations and expectations in his behavior field, and these in turn may lead to more precise differentiations and beliefs in his matrix relative to subtypes of restaurants and relative to subtypes of food, respectively, to be obtained from such restaurants and relative to the goodnesses or badnesses of these foods for producing hunger gratification. On subsequent occasions in which he is presented with the same stimuli these may lead to a more precisely differentiated and, we may hope, a more veridical behavior space.

Such learning takes place, I believe, not primarily through reinforcement in the Hullian sense but merely by the repeated "confirming" experiences of finding what restaurants lead to what foods by what costs and with what degrees of final gratification.<sup>4</sup> Further, this learning will through the laws of generalization spread to other similar types of restaurants and to other similar types of foods. The individual's matrix may, through the principles of generalization, become expanded to include positive and negative values for new, but more or less *similar*, types of restaurants and foods.

To sum up, a very abbreviated description of learning would be as follows. On the basis of past experience the individual brings a modal belief-value matrix to any new stimulus situation. This modal matrix becomes activated

<sup>4</sup> Such learning will, no doubt, be somewhat faster up to a limit with increasing hunger. Too great hunger will, on the other hand, probably interfere with learning. *That* food and *that* restaurant will come to be "gone to" more frequently which produce the greater satisfaction with the least cost. But this does not mean that the character and location of the "good" restaurant are necessarily going to be learned any faster than the character and location of the "poor" restaurant. In other words, there is no evidence that greater "reinforcement" will cause the one restaurant and the one food to be learned more rapidly than the other less good restaurants or less good foods.

and particularized as a result of the presented environmental stimuli and the specific need arousals at the moment. This activated matrix together with the environmental stimuli lead to a particular behavior space. This behavior space will result in locomotions of the behaving self in this behavior space and these locomotions will result in actual behavior. And this actual behavior will lead to new stimuli which will either confirm and tend to strengthen, or not confirm, and hence tend to modify the original object perceptions and expectations and position relation perceptions and expectations which constituted the original behavior space. Then these confirmed or modified perceptions and expectations will react back upon the matrix and either strengthen the differentiations and the beliefs already there or create new ones.

Next I should like to raise a question concerning the nature of the so-called secondary needs. Consider, for example, "need-achievement." It is obvious that in our own culture there is a large amount of something which has come to be called by this name whereas in certain other, more primitive cultures, there is reported to be much less of it. How are we to envisage this need-achievement? We shall probably all agree that this "propensity to achieve" is, to begin with, a means-activity—that is, that it corresponds to a set of acquired instrumental beliefs and values. The individuals have learned, i.e., have acquired means-end beliefs to the effect that, if they persist in and complete difficult tasks, they tend to get praised and that, if they give up and do not complete such tasks, they tend to get blamed. Assuming some simple and probably innate need, such as need-affiliation, then the receiving of praise can be conceived to lead to a direct gratification of this need, and the receiving of blame to a direct deprivation of it. Then as a result of these experiences of completing or achievement leading to praise and of not completing or giving up leading to blame, the individuals gradually acquire two new dimensions in their matrices: a completing dimension and a not completing dimension. Positive instrumental values will get attached to the cell or cells along the completing dimension and negative instrumental

values will get generalized to the cells along the non-completing dimension. In other words, the individuals can be assumed to build up a matrix like that diagrammed in Fig. 4.

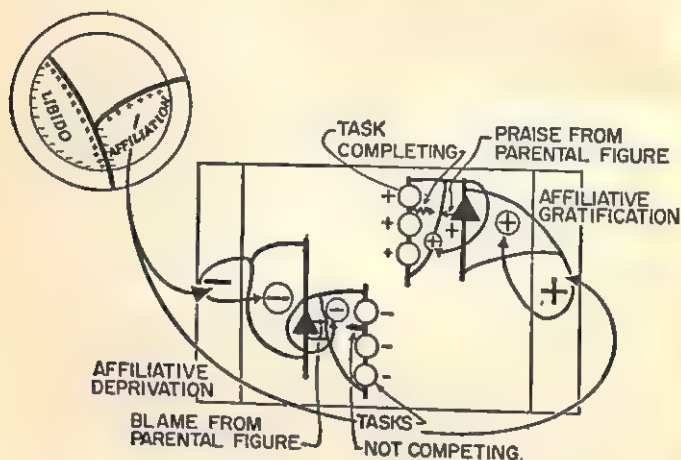


Fig. 4. Completing tasks and not completing tasks are believed to lead to parental praise and blame, respectively, and these latter, respectively, to gratification and deprivation of the affiliative (love) need.

Furthermore, it would appear that by the time they are adults, individuals who have been trained in a given culture where praise and blame were consistently, or perhaps inconsistently,<sup>5</sup> handed out for persistence and completing, on the one hand, and for giving-up or not completing, on the other, will tend to continue to "go in" for achievement, even though nobody appears any longer to praise or blame them for it. This suggests a further possibility in terms of our model: namely, that in time a really new need sac—to be called need-achievement—gets set up in the need systems of such individuals. This possibility is diagrammed in Fig. 5 which includes a new "functionally autonomous" need achievement.

<sup>5</sup> This latter possibility has been suggested by McClelland (4).

If this state of affairs is in fact finally arrived at, it would mean that such individuals should, and would, keep on "achieving" even though neither actual persons nor imaginary ones gave them approval and also even

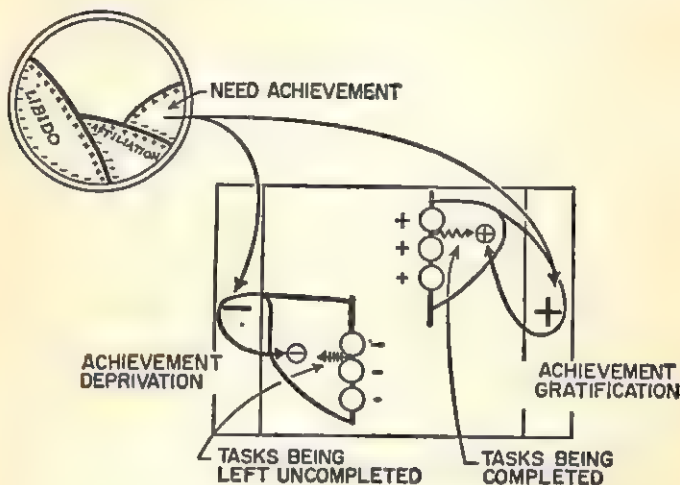


Fig. 5. Contents of need system and matrix on the assumption of an acquired, functionally autonomous, need achievement.

though no viscerogenic or other constitutional needs were especially furthered by such continued achieving. It would mean that these individuals, if placed on an island paradise where work, as such, was no longer valued and where it was not necessary to do much work to satisfy the bodily wants, would nevertheless continue to go in for achievement *per se*. To decide whether the set-up represented in Fig. 4 ever thus really turns into that represented in Fig. 5, many actually controlled observations would have to be made. Furthermore, if it did turn out that a truly new need such as this need achievement were generated, careful observations of the original conditions which favored the building up of such a new need in the one individual but not in the other would also have to be made.

My own personal hunch, at the present time, is that no such new need sacs are really established, but rather that all we really get are very powerful instrumental values connected by many and relatively strongly acquired belief lassos to a wide variety of ultimate, basic gratifications. If the basic needs could be satisfied without the activities involved in these so-called new needs, the latter would drop out.

Another point:—Some psychologists, who are greater students of social interaction than am I, may feel that I have not allowed sufficiently for the fact that the objects which appear in the behavior space and which get generalized in the matrix are not merely inanimate objects such as restaurants, foods, and the like, but also other persons and the complementary responses of these other persons. My answer would be that all expectations and beliefs, even those of the rat, include from the very beginning the resultant complementary responses of the other objects, whether these latter are animate or inanimate. The expectations and beliefs of "ego" in relation to "alter," whether that ego be rat or man, are expectations and beliefs as to what ego will receive in the way of return reactions from alter, whether such alter be a person or an alley wall.

Still another point: I do not have space here and I have not thought it through far enough to be too assertive about it, but I believe that the so-called Freudian mechanisms can be treated most fruitfully not as dynamic peculiarities in the need system or in the matrices of the subject but as relatively inevitable types of resolutions of conflicts in the concrete behavior space when the behaving self is simultaneously pushed both towards a given object and away from it. The type of defense mechanism that will result will depend more upon the nature of the objects and their relative then-and-there positions in the behavior space than upon any innate predispositional feature in the need system or in the belief-value matrices of the given subject.

Finally, I believe that very fruitful analyses of the distortions of the perceptual behavior space which are appearing in recent experiments such, for example, as those of Bruner and Goodman (1) in which, for example, in

poor children the more valuable coins were actually seen as larger (this has been shown by Rosenthal [5] to hold only for certain ages under certain special conditions), can be most usefully conceived as a case in which both a matrix which arrays the coins according to their monetary worth and a matrix which arrays the coins according to their physical sizes both project onto the same behavior space. This possibility is diagrammed in Fig. 6. The locomotion of placing the coin relative to size

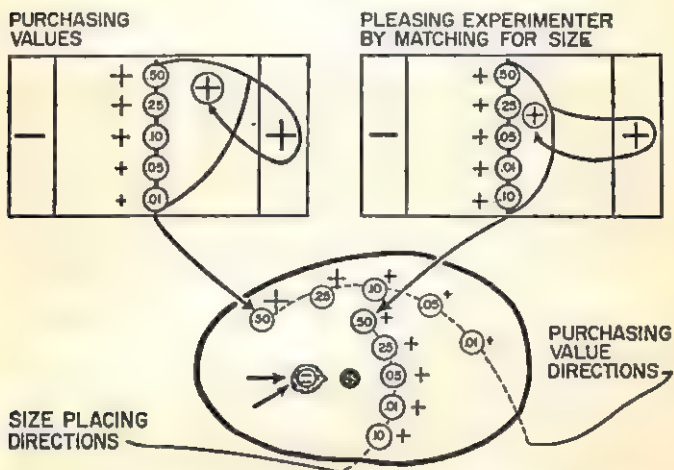


Fig. 6. Suggested explanation of Bruner and Goodman results. Matching of five-cent piece for size distorted by simultaneous (unconscious?) matching for purchasing value.

is distorted by a placing of it relative to purchasing power.

One last word: I should like to emphasize that it is my firm belief that all of the so-called dynamic problems of personality psychology and the resultant phenomena of selectivity of perception, cognitive distortions, and emotional break-downs, as well as the problems of the maintenance or disintegration of social systems and of cultures, will be illuminated by the general concept of the belief-value matrix. The way in which whole matrices,

whether in personality systems or in social systems, govern part matrices; the ways in which these part matrices do or do not conflict with one another and, finally, the ways in which such conflicting matrices lead to resultant concrete behavior spaces with resulting distortions and consequently "inappropriate" or "maladjusted" resultant locomotions and final behaviors—these are the ways in which I, at least, would attempt to conceive and to state the basic and most important problems for the understanding of society and of the individual.

### References

1. BRUNER, J. S., and GOODMAN, C. C. Value and need as organizing factors in perception. *J. abnorm. soc. Psychol.*, 1947, 43, 33-44.
2. GEWIRTZ, J. L. An investigation of aggressive behavior in the doll play of young Sac and Fox Indian Children. *Amer. Psychol.*, 1950, 5, p. 294.
3. HOLLENBERG, E., and SPERRY, M. Some antecedents of aggression and effects of frustration in doll play. *Personality*, 1951, 1, January.
4. MCCLELLAND, D. C. *Personality*. New York: William Sloane Associates, 1951.
5. ROSENTHAL, B. G., and LEVI, J. H. Value, need and attitude toward money as determinants of perception. *Amer. Psychol.*, 1950, 5, p. 313.
6. SEARS, P. S. Doll play aggression in normal young children: Influence of sex, age, sibling status, father's absence. *Psychol. Monogr.*, 1951, 65, No. 6.
7. SEARS, R. R. Effects of frustration and anxiety on fantasy aggression. *Amer. J. Orthopsychiat.*, 1951, 22.

## Problems Presented by the Concept of Acquired Drives<sup>1</sup>

JUDSON S. BROWN

*State University of Iowa*

It is perhaps safe to assert that in every serious attempt to account for the behavior of living organisms, the concept of motivation, in one guise or another, has played a major explanatory role. But it is not safe to assert that students of behavior have reached appreciable agreement as to how drives can be most meaningfully defined, what mechanisms are involved in each case, how many drives there are, or precisely how drives function as behavior determinants.

It is in the area of the biogenic or physiological drives that the major efforts of experimental psychologists have been concentrated during the past several decades; and it is not surprising, therefore, that level of achievement and amount of agreement is highest there. Considerable progress has certainly been made, notably in the case of the hunger, thirst, and sex drives as they are revealed in animal behavior.

But there is a substantial group of psychologists, who, though they might applaud progress in the understanding of biogenic drives, minimize vociferously the importance of those drives for the interpretation of adult human

---

Reprinted from *Current Theory and Research in Motivation*. University of Nebraska Press, 1953, with permission of the author and publishers.

<sup>1</sup> The writer is grateful to his colleagues in the Department of Psychology and in the Iowa Child Welfare Research Station for their stimulating and critical comments on the material presented in this paper. Additional thanks are due to Dr. I. E. Farber for a careful reading of the manuscript and to Dr. Julia S. Brown for several positive contributions to the final sections.

behavior. Part of the motivation behind this attitude may stem from a desire to elevate man to a unique, emergent position on the phylogenetic scale. Biological drives are regarded as too crass for an organism as noble as man. But a more important reason for this attitude, and probably a more defensible one, is that in a well-fed culture like ours, intense degrees of such drives as hunger and thirst are aroused too infrequently to bear a heavy explanatory burden. And to this may be added the fact that doctrines which hold instinctive sources of motivation to be of significance for human behavior have all but vanished from the psychological scene. These and other influences have led many to the belief that the important human motives are produced by learning during the processes of socialization and acculturation.

But to say that human motives are learned contributes little to our understanding until the details of the learning process have been clearly outlined in each instance. We must be able to specify precisely what has been learned, what conditions affect the acquisition and retention of the learned reactions, and how such motives operate as determinants of behavior. With the exception of recent work on fear, few details of this sort have been filled in for the acquired drives. Many writers speak glibly of acquired drives for money, for food, for specific liquids, for prestige; but they fail completely to specify the variables and processes involved in the acquisition of such motives. As a consequence, almost no two writers agree on answers to the questions of whether habits can become drives, whether learned drives can become functionally autonomous, whether incentives arouse drives, or which learning paradigms can be most meaningfully applied to motives.

An examination of contemporary discussions of motivation suggests that one of the major sources of misunderstanding is the failure of most writers to distinguish clearly between drives or motives, on the one hand, and habits or reaction tendencies on the other. When psychologists first became concerned with drives, they dealt almost exclusively with the unlearned, biogenic drives; and since the gap between such drives and learned reactions was large, little confusion resulted. But when

they began to speak of *acquired* drives, which embody the characteristics of both a learned reaction and a motive, precision of expression faded and the concepts of drives and habits lost their individualities.

For a psychology that seeks to discover lawful relations between environmental events and reactions, whenever an organism learns something, that something attains the systematic status of a response. Moreover, the process of learning is described as the formation of a new association between the designated response and some particular environmental event. On this view, if an organism can acquire a drive, then whatever is acquired qualifies as a response. But the response must be more than a response if it is also to be classified as a drive. Thus to have an acquired drive, is to learn a response possessing the characteristics of other responses and, in addition, the capacity to act as a drive. Since the higher organisms normally learn a great many responses, it is imperative that reactions having the added function of drives be clearly distinguished from those that do not. If drive is an important concept in its own right, and if it is indeed different from learned reaction tendencies, then this kind of classification should be both possible and desirable. But if drive is not a different concept, then there is clearly no need for employing two different terms.

### FUNCTIONAL PROPERTIES OF DRIVE

Apparently, then, it is especially easy, when dealing with learned motives, to confuse the processes of learning and of motivation; and because of this, it is especially important that an effort be made to keep them distinct. In order to do so, however, one needs a reasonably clear conception of the several ways in which a drive, whether biogenic or acquired, functions as a determinant of behavior. As an aid to the organization of our thinking let us consider at this point several functional properties of drives that seem to be of major importance.<sup>2</sup>

<sup>2</sup> Although this discussion of the functional properties of drives and the reasons for their introduction by psychologists contains no explicit references to the work of others, no pre-

1. *Drives function in combination with existing reaction tendencies to produce overt behavior.* This property is usually termed the *energizing* or *activating* property of drives. It refers to the fact that if an organism has a learned (or unlearned) potentiality for behaving in a specific way in the presence of given cues it will be more likely to exhibit such behavior when a drive is present than when it is not. Moreover it implies that mere reaction tendencies (associative predispositions, habits, cognitions) can never, in the absence of drive, culminate in overt behavior. As this property is usually described it includes no reference to a capacity to direct or steer behavior.

The tendency to attribute this nondirecting, energizing function to drives has grown naturally out of observations of a number of behavioral phenomena. Of particular importance, is the frequent observation of a *marked disproportionality between the energy content of a stimulus and the energy expended in the response*. A whispered warning at a time of danger may be followed by intensely effortful escape reactions; the faint creaking of a stair in a haunted house may lead to exceedingly vigorous action. Such behavioral relations have many physical analogies. For example, the disproportionality between the minute energy required to press the trigger of a gun and the tremendous energy released thereby from the cartridge. In all such instances some concept of a driving or moving force seems to be demanded; and in none of them is the energizing agent endowed with directing or steering functions.

The psychologist's "drive" to speak of the energizing function of drive is also aroused by observing that a single organism will, in exactly the same stimulus situation, behave differently on different occasions, and by observing that supposedly identical organisms also will react differently to identical conditions. A rat may eat slowly on one day, rapidly on another, and not at all on still another. If on the basis of independent evidence,

---

tense of originality is made by the present writer. An attempt has been made simply to rephrase and regroup certain ideas long prevalent in the literature on motivation and learning.

we can be confident the rat "knows how" to eat, and if this "know how" can be assumed to remain constant from time to time, we cannot attribute the variations in its behavior to fluctuations in its cognitions or reaction tendencies. As a consequence, *we may find it useful to invoke different degrees of hunger to explain the observed variability.* Of course, not all instances of the occurrence or nonoccurrence of a given response *must* be explained by the introduction of drive even though the environmental conditions yield nothing in the way of significantly related stimulus variations. In many cases it might be more sensible to appeal to structural anomalies, inhibitory states, competing reactions, or inherited capacities.

To complete the picture, it may be noted that sometimes an energizing drive is introduced to explain *response constancy in the face of normally effective variations in environmental conditions.* Let us imagine that three different foods are fed to three different rats, one food to each, and that all eat at the same rate. Suppose further that on previous occasions the three foods had always been eaten at different rates. These results, if genuine, might be explained by introducing individual hunger drives of sufficient strengths to compensate for the expected differential effects of the three foods. Less-preferred foods would be eaten at the same rate as more-preferred ones if hunger were greater when eating the less preferred ones.

It is important to point out that the drive property under discussion here has been phrased in such a manner that the drive can always activate reaction tendencies and yet lead in some instances to a decrease rather than an increase in overt activity. As a case in point, it is well known that rats when placed in open field situation often show marked signs of fear such as excessive defecation and urination. Their overt skeletal activity, however, may be markedly reduced, the animals sometimes becoming completely immobile. Thus the strong drive of fear seems to lead to inaction rather than action. We would disagree, therefore, with the frequently expressed view that drives always lead to more vigorous overt activity. What they *do*, is to act in concert with reaction tendencies to

produce whatever behavior is called for by the tendencies in conjunction with the specific environmental situation. If the dominant habit tendency is to freeze or to feign death, then this is intensified or energized. But if a tendency to escape, or do something else, is dominant, then this will be activated by the drive.

2. *A reduction in drive following a response will function under special conditions to increase the probability that the response will occur again in the same situation.* Since this is simply the familiar functional property of reinforcement, it seems unnecessary to comment upon it at length. It should be noted, however, that there is nothing in the above statement to indicate that *all* responses are learned because of drive reduction or that a given response is *always* learned if followed by drive reduction. The qualifying phrase "under special conditions" is included to take care of possible exceptions. Thus if a rat upon first traversing a maze is allowed to eat to satiation in the goal box, it may show no improvement in performance if re-introduced immediately into the starting box. It may go to sleep. Improvement can only be shown by a "special condition" such as providing a day's delay between the first and second trials. Other special conditions include such variables as the time between the response and the reduction in drive, the emotional state of the organism at the moment, amount of drive reduction, and rapidity of drive reduction.

3. *An abrupt increase in drive following a response will function under special conditions to reduce the probability that the response will occur again in the same situation.* This is the typical picture presented by punishment, and the range of conditions under which the property can be demonstrated is even more restricted than in the case of the preceding drive-reduction property. Thus, although abrupt increases in pain can be brought about following a response by the use of almost any intense stimulus, comparably quick increases in the hunger or thirst drives cannot be realized. We need not, at this time, concern ourselves with the mechanisms responsible for, or involved in, the realization of this functional property. The older view that punishment or pain as such weakens S-R bonds has few advocates at present.

But it is still empirically true that a response followed by pain tends to be abandoned. This may be attributed to the elicitation of an incompatible response by the punishment and the reinforcing of that new response by the cessation of punishment. In so far as this interpretation is applicable, this third property of drives becomes identical with the second. The same phenomenon might also be explained by assuming that fear becomes conditioned to cues antedating the noxious stimulus. Subsequently, responses of avoiding or withdrawing from those cues would tend to be strengthened by the consequent reduction of fear.

### SOME ASPECTS OF THE DRIVE-HABIT CONFUSION<sup>3</sup>

In the preceding discussion, the desirability of making a clear distinction between drives and habits has been emphasized and three functional properties of drives have been suggested as an aid to the making of such a distinction. This point of view, however, can scarcely be said to be generally characteristic of present-day psychology. The majority of writers in the field of motivation, though they use the two terms, not only seldom stress their separate functional properties, but often speak of drives as if they have the same guiding and directing functions usually attributed to habits. Allport (1), Leeper (14), McClelland (16), Krech (12) and Young (25), to mention but a few, all maintain that drives or motives function to direct and organize behavior as well as to impel it.

By way of contrast, the position defended here is simply that *every case of directed behavior is to be ascribed, not to drives or motives, but to the capacities of stimulus cues, whether innate or acquired, to elicit reactions*. The property of directing or guiding behavior was deliberately omitted from the group of drive properties that has just been considered.

\*The word *confusion* is used here to mean simply a blending or mixing leading to indistinctness. It is not intended to carry connotations of an approbrious nature. The term *habit* is employed in a rather loose, general sense to cover any associate predisposition whether learned or unlearned.

The confabulation of drive, habits, and rewards, is especially evident in interpretations of token-reward studies. This becomes apparent initially when one notes that many general treatments of learning and of learnable drives discuss the well-known studies of Wolfe (24) and of Cowles (5) under the heading of *acquired drives*. Some authors who do this seem to recognize the attendant difficulties and attempt to sidestep the issue of whether such studies demonstrate acquired drives or simply habits. Thus Miller (18) in his chapter on *Learnable drives and rewards* in the *Handbook of Experimental Psychology* presents the results of 21 studies of secondary rewards in a section entitled "Learned rewards and drives based on food and hunger." But in his tabular summary of the principles demonstrated by these studies, not a single study is cited as providing evidence for an acquired drive. Deese, however, who also treats the token-reward studies as examples of learned drives says, in reference to Wolfe's studies that "... the chimpanzees showed that they had developed a secondary drive for poker chips." (6, p. 95). But on the following page he speaks of "*The token-reward habit* in Wolfe's experiment . . ." (Italics by the present writer). Clearly there is indecision here since the same bit of behavior is described as a *secondary drive* on one page and as a *token-reward habit* on the next. From the present point of view, it is necessary to conclude that although tokens or other stimulus objects can serve as reinforcements for the learning of new responses, this fact alone does not constitute adequate grounds for introducing a new acquired drive for tokens. Such behavior can perhaps be explained by recourse to learned reaction tendencies, on the further assumption that the tokens acquired their secondary reinforcing power through their previous association with food. The pressure to introduce a "token drive" would obviously be much greater had the chimpanzees demonstrated a willingness to work for the chips when not hungry and when the chips were not exchangeable for food.

The temptation to endow drives with response-steering functions is especially difficult to resist when the steering stimuli are internal and when, in addition, there

is reason to suppose they have resulted from the same operations that produced the drives. Perhaps, therefore, a major reason for the common tendency to amalgamate drives and habits is that antecedent conditions leading to drive states may also lead in internal events exhibiting the functional (steering) properties of stimuli.

The assumption that functionally effective internal stimuli may accompany drives is of course a common one and its heuristic worth as an explanatory mechanism is, in some circles, well recognized. It seems worthwhile, therefore, at this point to digress briefly in order to list a few of the behavioral phenomena that have been interpreted by reference to these internal drive-accompanying stimuli. These are all, in a sense, manifestations of a single functional property of *drive stimuli*, viz., the capacity to lead to specific, directed reactions.

In the first place, conditions which are believed to bring about the arousal of a drive, seem on occasion, to lead also to the appearance of novel, relatively specific responses. And often, in such cases, no environmental stimuli can be found to which the new reactions can be traced. When this occurs in the apparent absence of adequate opportunities for learning, it may be assumed that the drive-arousing conditions have generated internal stimuli to which the novel responses are innately attached. Thus drive stimuli have been assigned a significant role in the production of new, perhaps highly patterned, responses (10, 18, 4).

Secondly, it has been assumed that any learnable reaction can become attached to whatever stimuli attend a given drive, and that different responses can also become associated with the stimuli of *different* drives (10, 11, 22). The first of these potential capacities is said to underlie the ability of an organism to make responses appropriate to a given drive. An adult organism, when hungry, probably seldom ingests large quantities of water by mistake if food is available. And because it does approach and ingest food, one may be tempted to endow it with a drive *for* food. What appears to be a preferable interpretation is this. The drive of the hungry organism is not a drive *for* food; the drive *qua* drive is here assumed to have only the three nondirective func-

tions mentioned above. Not drives, but *drive stimuli*, in combination with external events, are assigned the property of eliciting responses directed towards objects capable of reducing drives. In the case of the newborn infant, the period between birth and the first feeding presumably leads to hunger and to hunger stimuli. But until food has actually been ingested, it appears unwise to assume, with higher level organisms at least, that the drive stimuli have any marked capacity to elicit directed behavior. In this sense, the infant does not have a hunger for food, or for milk, or for any special kind of food. As it matures, however, it learns to make specific responses when hungry and these can become attached to stimuli attending hunger. It is these reaction-eliciting tendencies of drive stimuli that provide the directional characteristics often attributed to the drive.

The second of the above capacities, the association of different drive stimuli with different reactions, is exemplified by the classical studies of Hull (9) and of Leeper (13) in which rats were taught, in the absence of differential external cues, to make one response when hungry and another when thirsty.

A third explanatory role that can be assumed by drive stimuli, one originally proposed by Hull in his paper on *Knowledge and purpose as habit mechanisms* (8), is that of serving as an integrative mechanism. On this view, the presence of any persisting internal stimulus could function, through a kind of redintegrative action, to bring about the chaining of otherwise quite unrelated acts. All that would be required would be for each of the diverse reactions to become conditioned to the same internal stimulus.

A fourth example of the explanatory utility of internal drive stimuli is to be found in those instances where drives do not lead to more vigorous overt behavior or to performance facilitation. Here the drive-stimulus-elicited reactions are said to be incompatible with the responses typically evoked in a given situation (3). Their presence, as the result of an increase in drive, and an attendant intensification of the drive stimuli, could lead, therefore, to performance decrement rather than facilitation.

Finally, drive stimuli might be utilized in interpreting the apparent effects of drives upon the perception of external stimulus complexes. To take a hypothetical experiment, let us suppose that both hungry and satiated subjects are presented with near-threshold exposures of pictures of both edible and nonedible objects. From the results of recent studies of motivation and perception we might expect that the hungry subjects would be "perceptually sensitized" to the food pictures, and would consequently be more successful in identifying them correctly. Such results might be explained within the present framework as due primarily to the drive stimuli accompanying hunger. For obvious reasons, foods of various kinds are more likely to be seen when one is hungry than when one is not. And at such times considerable practice is obtained in learning to make appropriate verbal (naming) responses to the visual patterns provided by the foods. Consequently, the naming responses should come to be associated with a pattern of stimuli composed of both internal hunger cues and external visual ones. Were this the case, it would not be surprising if the hungry Ss, who possess both the internal hunger cues and the impoverished visual cues, should do better than the non-hungry Ss, who possess only the visual cues. On the present view, the hungry group might also be superior to the nonhungry group on nonfood pictures, though their superiority should be less than in the case of the food pictures. Facilitation of performance on nonfood pictures would be attributed to the energizing effects of drive per se, not to the presence of hunger stimuli. Perhaps some such mechanism as this, appropriately elaborated, would yield an adequate explanation of some instances of the sensitizing effects of motivation upon the response tendencies defining perceptions.

Returning now to the drive-habit confusion, a final point must be made. This concerns the role of verbal behavior in the tendency to postulate a different drive for each and every goal toward which directed reactions are exhibited. Human subjects are prolific in their use of phrases like "I *want* that object" or "I *desire* this" or "I *need* those." Since the subject *says* he has a multitude of needs, and since he did not say so at birth, the

unwary psychologist, especially if he pays too much attention to similar statements of his own, is likely to conclude that each such statement demands the postulation of a corresponding acquired motive. The mistake lies in the failure to realize that *these verbal phrases may indicate the presence and strength of reaction tendencies* rather than, or in addition to, the presence and strength of drives.

It is instructive, in this connection, to observe that the inclination to introduce a multitude of acquired drives for sought-after goals is minimal in reports of research on nonverbal animals. For example, if a rat has been repeatedly rewarded for turning left in a simple T-maze it will exhibit a strong tendency to approach the left alley. Typically this behavior is explained by saying that the response of approaching has become strongly connected to the cues of the left alley. The habit tendency is said to be strong, or the cognition, perhaps, is said to be very clear. But what is apparently seldom said is that the rat has a strong *left-alley-seeking drive*. Such a statement would probably be made, however, if the rat could talk, since it might, while in the starting box, exclaim, "I have a strong desire to get into that left alley!" Perhaps the "rat psychologist" has been spared some confusion because of the rat's inability to use words which can indicate the presence of either habits or drives, or both.

But let us take still another example. Nearly all will agree, perhaps, that the cessation of a fear-arousing stimulus, though it is an event, rather than an object like money, is empirically reinforcing. Certainly the accumulated data on animal experiments (20, 17, 15, 2) are convincing in showing that such a cessation, if it is sufficiently great, sufficiently abrupt, and sufficiently near in time to a preceding response, results in an increase in the probability that the response will occur on later occasions. Here, however, even though the event of stimulus-cessation might be described as a kind of goal, no one asserts that the fearful animal has a drive for stimulus cessation. To do so would be tantamount to postulating a *drive for drive-cessation*.

Up to this point, an attempt has been made to clarify

our understanding of acquired drives, primarily by insisting that the nondirective properties of drives be sharply separated from the directive functions of external and of internal stimuli. It is imperative, therefore, that we now examine some specific examples of supposed acquired drives in an effort to determine the possible utility of the distinctions that have been made.

#### THE SO-CALLED ACQUIRED DRIVES FOR MONEY, PRAISE, PRESTIGE, ETC.

It has already been noted that although many discussions of motivation assert the presence and importance of learned drives for such objects or goals as money, companionship, and eminence, no convincing and theoretically integrated accounts of the processes by which such drives are acquired and maintained have been presented. The acquired drive of fear stands alone as one for which the theoretical steps in the process of its acquisition and maintenance have been carefully worked out.

According to the formulations of Mowrer (19) and of Miller (17) the process of fear-acquisition is adequately described by the paradigm of classical conditioning. Thus if an originally neutral cue is repeatedly associated with a pain-arousing stimulus, the former will acquire the capacity to evoke an anticipatory form of the pain reaction. This premonitory response is termed fear or anxiety, and the functional properties of primary drives are assumed to accompany its arousal. It is also hypothesized that fear may carry with it distinctive stimuli to which new reactions as well as innate ones may be connected (15, 18). At one and the same time, therefore, fear is learned, it is internal, it appears to possess at least two of the nondirective properties of unlearned drives, and it has the capacity to generate response-steering stimuli.

This formulation appears to work rather well for fear; but in the case of the supposed drives for specific goal objects such as money, a similar paradigm is almost impossible to apply. What, for example, is the originally adequate (unconditioned) stimulus capable of arousing

a money-seeking drive (or response) so that it can become conditioned to a neutral stimulus? Obviously, the operation of deprivation will not work, since removing a person's money before he has learned of its value, cannot arouse a drive for money. If it did, we would be put in the position of maintaining that deprivation of something one doesn't want leads to a drive for that something. It seems likely, therefore, that the difficulties involved in attempting to apply the classical conditioning paradigm to any of the supposed drives for specific goal objects or situations, stem from our inability to denote appropriate unconditioned stimuli for the to-be-learned drives or responses.

It would be foolish to maintain, of course, that the paradigm for the learning of the so-called drive for money must be that of classical conditioning. Certain of the learning principles of the perceptual-cognitive variety might better serve to satisfy our need for an integrative schemata. Unfortunately, experts in the manipulation of such concepts have not, for the most part, been interested in acquired drives and have made no serious attempts to clarify these obscurities.

It is the writer's belief, however, that the problem of which brand of learning concepts to use is relatively unimportant and that the real roots of the difficulty lie deeper. Perhaps the greatest hindrance to progress lies in assuming the existence of drives where no such assumption needs to be made. We might advance more rapidly if we were to start afresh and deny at the outset that each and every object or situation for which an organism has learned to strive must be accompanied by a characteristic acquired drive for that object. To do so, of course, is simply to deny that acquired drives, when functioning as drives, have behavior-directing properties.

Suppose then, that people, in spite of what they say, do not acquire drives for any specific goal object whatsoever. What they can and do acquire, are multitudinous sets of habits or modes of responding to complex stimulus situations composed of both external and internal elements. And those reactions are reinforced repeatedly by objects or situations or events possessing acquired rewarding power.

Conceivably one might be able to account for all of the apparent instances of complex acquired drives by appealing solely to these habit phenomena. But most conceptions of behavior demand the presence of some kind of motivation if habits are to eventuate in acts. Let us consider, therefore, whether some source of *acquired* motivation can be uncovered to which we can sensibly attribute the general functional properties of drives.

One possible solution to our problem is to assume that the *important motivating component of many of the supposed acquired drives for specific goal objects is actually a learned tendency to be discontented or distressed or anxious in the absence of those goal objects.* On this view, stimulus cues signifying a lack of affection, a lack of prestige, insufficient money, etc., would be said to acquire, through learning, the capacity to arouse an anxiety reaction having drive properties. This learned anxiety would then function to energize whatever behavior is directed toward goal objects by stimuli, and its reduction, following the achievement of those goals, would be powerfully reinforcing.

Should such a proposal prove fruitful, it would lead to a substantial reduction in the number of acquired drives commonly deemed necessary. Moreover, it would satisfy our specification that drives must be nondirectional, while at the same time leaving room for the presence of internal behavior-steering stimuli. It might also simplify the task of detailing the steps in the acquisition process. For illustrative purposes, let us attempt to outline the principal elements of the process by which the anxiety underlying the so-called drive for money might be acquired.

During the first few years of a child's life there are innumerable occasions upon which the child experiences genuine pain. Its fingers get cut, burned, or mashed, it falls from chairs and tables, it catches childhood diseases, *ad infinitum*. And on nearly all of these occasions, normally solicitous parents behave in a worried, anxious manner. The frequent combination of pain with the cues provided by the sight of the worried parents, could result in the acquisition by those cues of the capacity to elicit anxiety reactions in the child. If such an associative con-

nection were very strongly established, the stimuli attending the behavior of anxious parents could then serve as the functional equivalent of an anxiety-arousing unconditioned stimulus in other situations.

Now if at the time anxiety is evoked by the parents' worried looks, they were to complain of being worried by a lack of money, the words, "we have no money," could become a higher-order conditioned stimulus with power to arouse the child's anxiety on subsequent occasions. Through the frequent repetition of the same or similar combinations of stimulating events the anxiety could become strongly connected to a multitude of phrases or conditions all having in common the meaning "lack of money." (We need not at this point, concern ourselves with whether mere contiguity is adequate for the formation of such associations, as Mowrer (21) would maintain, or whether some other factor such as drive-reduction is also necessary). After such learning has taken place the child will tend to become anxious whenever it hears phrases like "that costs too much" or "we can't afford it this month" or "we haven't the money for that." Each of these cue phrases might be individually conditioned to anxiety, though some might evoke the reaction via stimulus generalization or mediated generalization. By this mechanism any verbally announced phrase carrying the meaning for the child of "lack of money" or any equivalent stimulus, such as an empty purse, could become the exact counterpart of the fear-arousing conditioned stimulus in animal studies.

But if this is true, then the actual presence of money is the specific condition for producing the *cessation* of the anxiety-arousing cues. Having money in one's possession is equivalent to escaping from the white-box cues into the black-box cues in Miller's (17) classical experiment on fear. If not having money is the cue for anxiety, then having money is the principal means, though probably not the only one, for removing or obliterating the anxiety-arousing cues.

Following this line of reasoning further, it can now be asserted that any response, if it occurs during a period of anxiety resulting from insufficient money, is likely to be reinforced by the drop in anxiety attending the re-

ceipt of money. Money thus functions as a reward or reinforcement. Here, however, it is especially important to note that money has become a reinforcer, not because of any previous association with the reduction of a primary drive such as hunger, but simply because anxiety has become connected to cues of not having money, and because having money eliminates such cues.

It also seems likely that a variety of other events than money could also serve to reduce the anxiety aroused by cues denoting insufficient money. Any activity such as getting drunk, making love, eating, or whatever, that successfully directs the individual's attention away from the disquieting cues and thereby leads to a diminution of anxiety would tend to be reinforced. Moreover, such behavior would also tend to become connected to both the anxiety-precipitating cues and to the anxiety-generated cues. With sufficient repetition it might well become a habitual, though unrealistic, adjustment to lack of money. By this reasoning, it might be possible to explain a number of instances where activities or objects that have never been associated with the reduction of primary drives seem nevertheless to have gained the power of reinforcement.

The essentials of the foregoing interpretation may be summarized as follows. In many instances, if not all, where adult human behavior has been strongly marked by money-seeking responses there appears to be little need for postulating the operation of a learned money-seeking drive. One does not learn to have a drive for money. Instead, one learns to become anxious in the presence of a variety of cues signifying the absence of money. The obtaining of money automatically terminates or drastically alters such cues, and in so doing, produces a decrease in anxiety. Money-seeking responses, or other reactions, appearing during the arousal of anxiety are strongly reinforced by the decline of anxiety attending the receipt of money.

It seems worthwhile at this point to observe that the proposed method of dealing with some of the acquired drives is not at all novel, at least in its general structure. Tolman, for example, has outlined a somewhat similar mechanism in the case of what he calls the drive of gre-

gregariousness. Thus, he writes, "In *gregariousness* the individual, when separated from the flock or herd or group, seeks to get back into it in order to prevent an internal sufference which in gregarious species seems to result directly from lack of surrounding animals." (23, p. 21). Although Tolman regards this as an example of a biological, not a learned, drive, it is evident that his "internal sufference" is the functional equivalent of anxiety, and that it is aroused by cues indicating a deficit of a special kind. Dollard and Miller (7) have followed a similar line of reasoning in describing the anxiety of a child separated from its mother. According to them, the child responds to cues of being separated from the mother with the drive of fear, and its reactions of approaching the mother are rewarded by the fear relief attending her actual presence. Although these authors suggest that this mode of analysis might be extended to other acquired drives, only a single paragraph is specifically devoted to the possibility that fear or anxiety might be an important component of many socially learned motives. Moreover, they do not attempt to make the sharp distinction between drives and habits which is here regarded as essential to the further clarification of acquired-drive problems.

One aspect of this interpretation of the so-called "drive for money" upon which we have not yet touched, concerns the role that might be played by internal stimuli accompanying anxiety. In an earlier section of this paper the potential importance of stimuli accompanying unlearned drives was outlined in some detail. If discriminable internal stimuli also attend the inception of anxiety, and if conditions conducive to learning are operative, a wide diversity of behavior patterns could become attached to those stimuli. And predominant among those patterns, would be various responses of seeking money. Because anxiety might thereby lead, through the response-eliciting agency of its characteristic stimuli, to money seeking behavior, it would be easy to confuse its driving and response-eliciting function, and to assert that people have a "drive for money." From the present viewpoint, the observation of directed behavior at a time of anxiety arousal should not be ascribed to a new

drive for the object which reduces the insecurity. Rather, it should be explained by appeal to the cue stimuli attending anxiety and to the drive property of anxiety in activating the latent reaction tendencies attached to those cues.

Although the foregoing analysis has dealt principally with behavior of seeking or working for money, a similar interpretation might be made of the so-called drives for prestige, for affection, for eminence, etc. That is, it might be useful in these and other cases to deny that a unique drive must be postulated in each instance, and to assume instead a single drive of uneasiness or insecurity or anxiety common to all. Thus, cues signifying "lack of affection," "lack of achievement," or "lack of prestige," could acquire, through the process of learning, the power to serve as anxiety arousers. The attainment of affection or eminence would thereupon function to eliminate the anxiety-arousing cues, and consequently, anxiety.

By way of qualification, it should be observed that the proposed analysis does not apply necessarily to the behavior of animals in token-reward studies. The chimpanzee subjects in Cowles' and Wolfe's experiments can hardly be said to have been anxious in the absence of poker chips. Nor does it seem likely that the chips were rewarding because of their capacity to reduce anxiety. But if we deny the existence of both a specific drive for chips and an anxiety drive, to what can we appeal? Hunger appears to be the most likely candidate in this situation. In support of this selection, it may be recalled that the animals would not work for chips when satiated for food. This could mean either that they had no drive for chips, or, as Deese suggests, that the chip-drive was somehow specifically attached to hunger. Since the second of these possibilities requires the postulation of two drives, it seems less preferable than the first. If hunger is assumed to be the only functioning motive, we would not expect the habits of operating the test devices to be activated by the mere sight of poker chips under conditions of satiation. But such habits should be activated if hunger were present, and should continue in effective strength for some time even when the

immediate reward was not food, but chips. The capacity of the chips to function as rewards can be readily interpreted as an instance of secondary reinforcement. Brass tokens, not exchangeable for food, acquired no such rewarding power, and the regular poker chips lost that power when their insertion into the "chimpanomat" failed to produce food. Thus it could be maintained that the behavior of the chimpanzees was not attributable to a poker-chip getting drive, but to a hunger drive, and that new responses were learned because of the secondary reinforcing power of the chips and not because of their effectiveness in reducing a learned drive for chips.

It is quite likely that an identical interpretation is applicable to many cases of money-reinforced behavior in humans. Not all money-seeking behavior need be motivated by anxiety. It could be motivated by other drives such as hunger, or thirst, with the ultimately obtained money acting as a reward because of its close and frequent association with the receipt of food or water. This is probably the most common interpretation to be found in the literature, and both this mechanism and the proposed anxiety mechanism could well operate jointly in all instances of the supposed drive for money.

One final point with respect to the incentive problem. A number of writers in discussing the functions of incentives in behavior have been puzzled by the capacity of a lure—say, a banana seen at a short distance by a chimpanzee—apparently to serve as both a drive-arouser and, when actually eaten, as a drive-reducer. According to the position held here, this seeming paradox would be resolved by denying that the sight of a goal object can arouse a drive for that object. As we have repeatedly insisted, neither learned nor unlearned drives *per se* can be said to be directed toward, or for that matter, away from, any object. The supposed driving function of incentives would therefore be interpretable as an associative phenomenon, not as a motivational phenomenon. Such an interpretation would stress the fact that the goal stimuli, the incentives, have been repeatedly present, along with other stimuli when the organism has made approaching responses. These responses have been suitably rewarded, and hence the sight of the object be-

comes a part of the complex of stimuli eliciting approach strivings. Considered as such, the goal object is basically the same as any other stimulus that has acquired strong approach-response evoking tendencies. The only difference lies in the tremendous amount of previous over-learning that may have taken place in the case of goal objects such as food. When the food is actually in the mouth, it can no longer serve as a stimulus for approach. For one thing, the organism cannot see it anymore. It therefore loses its activity-arousing properties and becomes, instead, a stimulus that elicits eating and, perhaps, relaxation.

### SUMMARY

Many contemporary students of motivation hold to the view that the most significant of the human drives are not biological; rather, they are learned during the normal course of socialization and development. Unfortunately, the ardor with which this view is defended and the degree of confidence placed in its soundness far exceed the precision with which the details of the underlying mechanisms have been specified. In most instances, not even tentative paradigms for the learning of the supposed motives have been outlined in reasonably acceptable form.

Broadly viewed, this lack of adequate conceptualization may be ascribed to a failure to differentiate between drives as determinants of behavior and specific tendencies to react to particular stimulus situations. Of more direct significance, however, is the widespread failure to distinguish carefully between the characteristics of learned motives and the characteristics of other learned reaction tendencies.

In an effort to bring these difficult problems into sharper focus it has been suggested that the principal functional properties of drives, whether innate or acquired, are: (1) to activate or energize latent reaction (associative) tendencies; (2) to reinforce responses whose elicitation is followed by a reduction in drive; (3) to function as a punishment whenever abrupt increases in drive occur following a response. Any capacity to

elicit *directed* behavior is specifically denied to drives *qua* drives. Only those events, whether external or internal, that have the properties of stimuli are alleged to provide the occasions for directed reactions.

The popular contemporary inclination to ascribe response-steering properties to drives is believed to stem from (1) the possibility that conditions leading to drive arousal may also produce internal stimuli capable of directing behavior; and from (2) the fact that human subjects use verbal phrases whose manifest reference is to motives, but which, with equal reason, can be interpreted as reflecting the strength of habits.

In exploring the implications of these distinctions, certain of the so-called acquired drives for goals like affection, money, and eminence, have been discussed. Because drives, as such, are regarded as incapable of steering or directing behavior, it is necessary to deny that individuals can acquire drives for any specific goal object or situation whatsoever. What individuals do acquire are numerous habits or modes of reacting to complex situations made up of both external and internal stimulus components. In some instances, biological drives alone may be adequate to activate these reaction tendencies. But in others, it may be necessary to appeal to activating motivations that are themselves acquired.

As a tentative solution to this problem, it is proposed that the significant motivational component underlying many of the supposed acquired drives for particular goal objects may be a learned tendency to be anxious (discontented, insecure) in the absence of those objects. On this hypothesis, stimulus cues signifying a lack of affection, a lack of prestige, insufficient money, and the like, are said to acquire, through learning, a potentiality for arousing a state of uneasiness or anxiety having the functional properties of a drive. Thus this learned uneasiness would function to energize whatever behavior is directed toward the securing of affection, prestige, or money; and its reduction, subsequent to the achieving of those goals, would be reinforcing.

In order to provide a concrete illustration of this view, an attempt has been made to work out the details of the learning process by which the anxiety underlying

the so-called drive for money might be acquired. The suggested mechanism is coordinate with the paradigm of conditioning and with current conceptions of the acquisition of fear.

### References

1. ALLPORT, G. W. *Personality: A Psychological Interpretation*. New York: Holt, 1937.
2. BROWN, J. S. and JACOBS, A. The role of fear in the motivation and acquisition of responses. *J. exp. Psychol.*, 1949, 39, 747-759.
3. BROWN, J. S., KALISH, H. I., and FARBER, I. E. Conditioned fear as revealed by magnitude of startle response to an auditory stimulus. *J. exp. Psychol.*, 1951, 41, 317-328.
4. BROWN, J. S. and FARBER, I. E. Emotions conceptualized as intervening variables—with suggestions toward a theory of frustration. *Psychol. Bull.*, 1951, 48, 465-495.
5. COWLES, J. T. Food tokens as incentives for learning by chimpanzees. *Comp. Psychol. Monogr.*, 1937, 14, No. 5.
6. DEESE, J. *The Psychology of Learning*. New York: McGraw-Hill, 1952.
7. DOLLARD, J. and MILLER, N. E. *Personality and Psychotherapy*. New York: McGraw-Hill, 1950.
8. HULL, C. L. Knowledge and purpose as habit mechanisms. *Psychol. Rev.*, 1930, 37, 511-525.
9. HULL, C. L. Differential habituation to internal stimuli in the albino rat. *J. comp. Psychol.*, 1933, 16, 255-273.
10. HULL, C. L. *Principles of behavior*. New York: Appleton-Century-Crofts, 1943.
11. KENDLER, H. H. An experimental examination of the non-selective principle of association of drive-stimuli. *Amer. J. Psychol.*, 1949, 62, 382-391.
12. KRECH, D. Cognition and motivation in psychological theory. In *Current Trends in Psychological Theory*. Pittsburgh: Univ. Pittsburgh Press, 1951.
13. LEEPER, R. W. The role of motivation in learning: a study of the phenomenon of differential motivational control of the utilization of habits. *J. genet. Psychol.*, 1935, 46, 3-40.
14. LEEPER, R. Current trends in theories of personality. In *Current Trends in Psychological Theory*. Pittsburgh: Univ. Pittsburgh Press, 1951.
15. MAY, M. A. Experimentally acquired drives. *J. exp. Psychol.*, 1948, 38, 66-77.

16. McCLELLAND, D. C. *Personality*. New York: Sloane, 1951.
17. MILLER, N. E. Studies of fear as an acquirable drive: I. Fear as motivation and fear-reduction as reinforcement in the learning of new responses. *J. exp. Psychol.*, 1948, 38, 89-101.
18. MILLER, N. E. Learnable drives and rewards. In S. S. Stevens (Ed.) *Handbook of experimental psychology*. New York: Wiley, 1951.
19. MOWRER, O. H. A stimulus-response analysis of anxiety and its role as a reinforcing agent. *Psychol. Rev.*, 1939, 46, 553-566.
20. MOWRER, O. H. and Lamoreaux, R. R. Avoidance conditioning and signal duration—a study of secondary motivation and reward. *Psychol. Monogr.*, 1942, 54, No. 5.
21. MOWRER, O. H. On the dual nature of learning—a reinterpretation of "conditioning" and "problem solving." *Harv. educ. Rev.*, 1947, 17, 102-148.
22. SPENCE, K. W. Theoretical interpretations of learning. In C. P. Stone (Ed.) *Comparative Psychology*. New York: Prentice-Hall, 1951.
23. TOLMAN, E. C. *Drives Toward War*. New York: Appleton-Century, 1942.
24. WOLFE, J. B. Effectiveness of token rewards for chimpanzees. *Comp. Psychol. Monogr.*, 1936, 12, No. 60.
25. YOUNG, P. T. Emotion as disorganized response—a reply to Professor Leeper. *Psychol. Rev.*, 1949, 56, 184-191.





## The Nature of the Drive as Innate Determinant of Behavioral Organization

HENRY W. NISSEN

*Yale University and Yerkes Laboratories  
of Primate Biology*

One member of last year's Symposium (20) remarked that the graduate students at the University of Nebraska should be the world's most informed group of people on the subject of motivation—after all, you had the advantage of listening to the concentrated words of wisdom of six or more presumable experts in that field. Reading over what these experts had to say, it seems to me that you might also be the most confused and befuddled group in regard to this topic. Each speaker, individually, certainly made valuable contributions to the subject, worthy of your most thoughtful consideration, but—so it seems to me—each one stressed a different aspect of the whole without successfully relating it to other aspects. A unity, integration, or synthesis of motivational theory, corresponding to the unity of the motivated organism, I believe, was not achieved.

This state of affairs, however, should neither surprise nor discourage us. It is typical of scientific progress, or at least of the progress of scientific theory. Before we can synthesize on a large scale, the component parts to be integrated must themselves be properly organized.

In general, my exposition will proceed in stepwise fashion from the simple to the complex, from phylogenetically and ontogenetically earlier to later stages. Em-

---

Reprinted from *The Nebraska Symposium*, edited by Marshall R. Jones, Lincoln: Univ. Nebraska Press, 1954, with permission of the author and publishers.

phasis will be on primitive rather than complexly organized behavior, on animals more than on human beings, on individual rather than social behavior, and on the young rather than the highly experienced adult individual. This is in line with two of my pet prejudices; namely (a) that the genetic or evolutionary approach, which seeks to find explanations in origins, and which accounts for the present in terms of its historical development, has certain unique and self-evident virtues; and (b) that it pays to be patient, to be sure one's I-beams and rivets are sound before building a skyscraper. Psychology will profit in the long run by devoting time and effort to basic problems of description, carefully specifying the fundamental units with which it has to deal, before plunging ahead, hastily and precipitously, into the logical and mathematical elaboration of terms whose validity, "reality status" or relations to observable fact are still obscure. Motivation is part of explanatory rather than of descriptive psychology, but our explanations will fall flat if the descriptive units or categories to which they refer prove unreal or invalid. A corollary of this prejudice is that before investing all our time and effort in problems of learning or behavior modification, we should determine carefully what the behavior is before it is modified.

My aim is to be constructive, but as is often the case in the course of building something new, we must also clear away the rubble. In particular, there are four rather prevalent notions which have intruded themselves into our thinking about motivation and which must be gotten out of the way: (a) The notion that a useful classification of behavioral determinants can be made on the basis of whether the effective stimuli are internal or external. This idea was emphasized in last year's symposium by Dr. Harlow (8), who rightly pointed out that behavior is determined as much by objects and features of the environment as by internal physiological states. This fact, however, does not mean, as the speaker seemed to imply, that behavior is *either* externally or internally determined. I shall try to show that in all behavior there are predisposing *and* precipitating causes, sensitizing and eliciting-directive components of determination,

and that *both* elements may be internal, both external, or one internal and one external. The nervous system, in its virgin or experientially modified state, is of course always to be reckoned with as an internal determinant. (b) The notion that there are two *kinds* of drive, biogenic and psychogenic, or homeostatic and nonhomeostatic. It is my belief that such a distinction is neither defensible, nor necessary, nor helpful; that, contrariwise, the distinction gets us off in the wrong direction at the very start of our analysis. (c) Intimately related to the foregoing, is the notion that drives may be divided usefully into those which are primary or innate, and those which are secondary or derived. Insofar as the term "drive" is helpful at all—and I do believe that, whatever we may call it, the concept of drive is of central importance,—all drives are innate. That, at any rate, will be my contention. The complications of motivational organization derive from learning signals, from acquiring new goals, or rather subgoals, and not from learning or creating new motivational mechanisms coordinate with those, usually called homeostatic, which are inherent in the organism and which are synonymous with or completely implied in the concept of life and, therefore, in that of evolution. (d) The notion that all behavior is organized and motivated by a limited number of major drives. Observation of behavior in free as opposed to experimentally restricted situations indicates, I believe, that a large portion of behavior consists of elements, or unit acts as I shall call them which are not part of a larger system but are autonomously motivated.

These rather bald statements of the critical side of my discussion will be developed further as we proceed with our more positive and constructive aims.

#### INTERNAL AND EXTERNAL DETERMINANTS

If we were to be asked the question, "Is *all* behavior motivated?", I think most of us would tend to respond immediately and without hesitation "Yes, of course." We might go on to point out that nothing happens without cause, and that the effect, which we call be-

havior, must have a cause, which we call its motivation. If our questioner were to probe further and ask whether the beating of our hearts and the secretory activity of our glands were examples of motivated behavior, we might hedge that the heart-beat is a "part-activity" which is the province of physiology rather than of the psychologist who is interested in behavior of the organism-as-a-whole. We might also say that the causation of heart-beating is not the kind to which the concept of motivation is usefully applicable. But our questioner might continue: "Salivary secretion, then, is a part-activity which is not really behavior, and it is therefore inappropriate to ask whether Pavlov's dogs were motivated." By this time some of us would be embarrassed. Are the contractions and expansions of our blood-vessels "behavior," and are they motivated? How about the lid-reflex and the knee-jerk; the geo- and helio- and stereotropisms or taxes of the lower organisms? For that matter, how about finger or foot withdrawal from electric shock or heat? Are these not behaviors, and are they not motivated? Some of them are part-activities and some are activities of the organism-as-a-whole. Most of them are determined externally and internally; that is, their form, and whether they occur at all, depend on external stimuli and internal states.

The temperature and chemical constitution of the medium determine whether certain of the lower organisms will or will not respond to the onset of light. The direction of the light rays then determines the form of the response—that is, whether the organism orients upwards or downwards (28, vol. II). When an alligator is placed on its back, none of the usual responses to visual and tactual stimuli occur; the animal remains completely limp (28, vol. III). Drugs and hormones of various kinds may accentuate, eliminate, or reverse the customary responses to external stimuli. According to Lorenz (15) the mere lapse of time since the last previous elicitation of an innate movement pattern (*Erbkoordination*) may cause that pattern to appear, without reference or orientation to anything in the environment. You will recall that he terms this a *Leerlaufreaktion*, or reaction *in vacuo*. Anyone who has observed the sucking

movements of a sleeping human or chimpanzee infant, which occur as feeding time approaches, will realize that the *Leerlaufreaktion* is more than a figment of Lorenz's imagination. And we know that response to an external stimulus often depends on what we call the "deprivation interval"—the lapse of time since the last previous feeding or sexual activity.

#### THE PREDISPOSING DETERMINANT:

##### SENSITIZATION LOWERS RESPONSE THRESHOLDS

Let us consider now the implications involved in the occurrence of such simple forms of behavior. Most obvious, perhaps, is the variability in the threshold of response. The organism behaves like a machine so constructed that certain stimuli produce certain movements, under certain conditions. It is these conditions which determine the threshold of response. If the response is independent of all conditions, and occurs to the stimulus under all circumstances, we have what corresponds to the textbook definition of a reflex or, as it is inappropriately called, an "unconditioned reflex." (As you all know, a truly unconditional reflex is an abstraction which has yet to be discovered in the living organism.) Conditions may either raise or lower the response-threshold, but usually we are more interested in the factors which heighten responsiveness, and I have elsewhere (22) suggested the term, "sensitizing component of behavior determination" for designating these threshold-lowering factors.

As we have already seen, such internal conditions as hormones and nutrients circulating in the blood-stream may serve as sensitizers, increasing responsiveness to certain stimuli. Whether such apparently external conditions as temperature or chemical constitution of the medium should better be thought of as internal conditions—perhaps affecting the metabolism of the organism and thus raising or lowering thresholds—may be questioned. Changes in responsiveness determined by light, olfactory or tactual stimuli qualify less ambiguously as external conditions. Finally we must note that past experience, in the widest sense of the term and not only

in the sense of learning, may sensitize, or desensitize the animal. Here we are thinking of phenomena such as those called adaptation and temporal summation. A stimulus repeated frequently at short intervals may eventually fail to elicit its response. Contrariwise, repetition of a faint stimulus which at first evokes no response may finally produce the response. Again it may be questioned whether such sensitization effects should be ascribed to external or internal conditions. Since the distinction is so difficult if not impossible to make, it seems profitless to attempt to designate sensitizing effects as exclusively external or internal.

### PRECIPITATING DETERMINANTS

#### ELICITING AND DIRECTIVE STIMULI

In addition to the predisposing causes of behavior, which we have described in terms of thresholds raised or lowered by internal or external conditions, there are precipitating causes. The response must be released or elicited. When an animal is thirsty, it does not perform the act of drinking unless and until water is actually present. The physiological disequilibrium called thirst sensitizes drinking responses; the stimulus of water elicits them. In the case of the so-called reflex, it is difficult to distinguish between the sensitizing and releasive components of behavior determination. It may be that either the same condition or stimulus serves both functions, or that the threshold of response is chronically so low that further sensitization is not necessary. The blink reflex, for instance, occurs almost invariably, and very quickly, when the proper stimulus is applied. The concept of the *Leerlaufreaktion*, on the other hand, requires no releasive stimulus at all; with the passage of time since the reaction has last occurred, the action-specific energy for it piles up until a threshold is reached and the reaction takes place without further stimulation. This is the extreme case. As Lorenz (15) points out, the response is more often precipitated not in a complete "vacuum," but by a stimulus more or less similar to the ordinarily effective one. Thus, under the "pressure" built up by

sexual deprivation, the copulatory act of certain birds may be elicited by an animal of the same sex, by an immature bird, or even by the finger of the experimenter which only remotely resembles the usual and biologically adaptive object of sex activity. It seems to me that hunger contractions, occurring periodically without the added stimulus of food seen, smelled, tasted, or in the stomach, may be thought of as a striking instance of the *Leerlaufreaktion*. Although, therefore, the releasive or eliciting factor is typically an external stimulus, it is not always or necessarily so. Even in a constant, unchanging environment—let us say a cage in a sound-proof air-conditioned room—the animal will alternately rest and pace; it must be that internal conditions or stimuli change from time to time so as to elicit one or another response.

So far we have talked about responses, or better, movements, without considering their reference to the environment. Such movements as hunger contractions seem to have no reference or directionality towards the environment whatsoever. Many other movements or movement patterns such as blinking, coughing and sneezing may get rid of an irritating stimulus and so affect the organism-environment relationship. But the "direction" of such movements is pretty nearly fixed and invariable. Most of the behaviors in which we as psychologists are interested, however, are directed in reference to the environment or parts of it. Most of them, for instance, can be described as approaches to or withdrawals from an object or an area of stimulation. Insofar as this is the case, the response is oriented, and it is always the external stimulus—or more correctly, the spatial relation of the object or stimulus to the organism—which determines the orientation or directionality. For the moment we do not need to go further into description of the response. It is obvious that in addition to its positive-negative valence, its general character of being either an approach or avoidance, the response has a certain form. Thus approach may involve forward locomotion, manual touching, mouthing, grasping, pulling, and so on. Such details need not be considered at this point.

## DESCRIPTIVE CRITERIA OF THE DRIVE

So far we have not used the term "drive" in our analysis of simple behaviors. However, we have introduced the concepts of response threshold and of sensitization factors which influence that threshold. These concepts, I believe, form the essence of what we usually mean by drive. Let me list the features of behavior commonly accepted as demanding the concept of drive or its equivalent: (a) "Drive" is not usually found necessary in explaining very short and simple acts, such as those considered so far. It is applied, rather, to longer sequences of acts, which are patterned or organized in rather specific ways. "Drive" is a construct used to explain such organization. (b) Typical of drive behavior is that it is goal-directed or purposeful; the earlier parts of the sequence may be somewhat variable, but it culminates in a specific act or consequence, which often gives the sequence its name. (c) The patterned sequence is variably present or absent, depending on conditions such as deprivation and hormones. That is, it is subject to sensitization. (d) "Drive" somehow produces or makes possible instrumental learning.

It may be useful to compare these four criteria with the four (three positive and one negative) proposed by Dr. Brown (5) last year. His first one—that the drive "activates" or "energizes" latent associative tendencies—is roughly equivalent to our third one, concerning the sensitization of S-R's. His second and third criteria deal with the effects of drive reduction and increase; they concern the role of the drive in learning, as is less specifically stated in (d) above. Brown's fourth or negative characterization—that drives as such only "provide the occasion" for reactions and do not themselves direct behavior—is roughly equivalent to our distinction between sensitizing and eliciting-directive components of behavior determination. Our first criterion, concerning length or duration of behavioral sequence, is perhaps vaguely implied but not specified by Brown. Our second item, regarding termination of the sequence in a consummatory response or consequence, is again only hinted at by Dr. Brown.

## FUNCTIONS OF THE DRIVE

We have seen that even reflexes and taxes involve predisposing and precipitating conditions. In addition to such relatively rigid and invariable unit acts, there is to be seen especially in "higher" animals such as the primates, a vast array of movements—or brief, compact movement-patterns—which are quite variable in their occurrence. Particularly in younger members of the species, the stimuli which elicit them are obscure; different movements may occur to the same stimulus at different times, and the same movement may occur in response to various stimuli. We often speak of them as random or spontaneous activities; as squirmings, wriggings and babblings. Sometimes, adultomorphizing, we call them playful or exploratory. Early in life these variable acts are not closely and specifically associated with any particular sensitizing condition. It is true that the hungry baby is more active than the satiated one, but these activities may be pretty much the same whether the infant is hungry, in an uncomfortable position, has a wet diaper, or is merely full of oomph and go. That is to say, many acts in the repertoire of the young individual are free-floating—unattached to specific conditions or stimuli.

As the individual grows older, this chaotic state of affairs is gradually replaced by a greater uniformity, stability, and predictability of response. A given S tends to be followed oftener by a certain R than by other R's. Furthermore, some R's tend to occur only under specific conditions of sensitization; the same S which under one condition reliably elicits R1, under other conditions is disregarded, evoking no observable response at all, or it may elicit R2. We say that behavior is becoming increasingly integrated, patterned, or organized. These expressions, however, refer to two quite distinct and separable phenomena. On the one hand they indicate an increasing consistency or uniformity of simple S-R's, habits, or unit acts. A tickle in the ear no longer evokes screaming and an assortment of haphazard, undirected bodily movements, but instead a precise application of the little finger to the spot, or perhaps a swallowing movement.

Sight of the candy store evokes the question, "Daddy, can I have a dime?" rather than a running to the window, jumping up and down, and drooling. In Brown's terms, reactive or associative tendencies are formed.

Another, and distinct meaning of "behavior organization" is the integrating of a series of unit acts into longer sequences which may be quite uniform from one occasion to the next. If the evidence indicates that this uniformity or consistency is a function of species-constant, innately determined reactive tendencies, we may call the sequence an instinct, or perhaps better, instinctive behavior. If, on the other hand, the consistency is a product of experience, the sequence is termed a complex habit. The errorless running of a maze having many *culs de sac* is the classic example of such a learned routine. Each of a number of stimuli, which originally elicited variable responses, now reliably and consistently evokes always the same response. Just why or how this occurs is, of course, the central problem of learning, to which I shall return. For the moment I wish merely to emphasize that the problem of learning, of reinforcement versus contiguity versus cognitive maps, and so on, is part of the still broader problem of explaining how unit acts in general are integrated into uniform patterns.

In order to complete our survey of longer sequences of behavior, we should perhaps add two more categories to those of instinctive and habitual patternings. The first of these are integrations which analysis indicates are products of experience, but not of experience with the specific stimuli or situations which were involved in previous instances. They are the result of what we refer to as "higher mental processes" such as transfer, reasoning or insight. For present purposes we may content ourselves by saying that they require the capacity to generalize, to recognize similarities, and to take into account stimuli not presently available to the senses. Since this discussion is about motivation rather than about cognition, I shall not attempt to go further with the analysis of this kind of process. As a fourth category there are those sequences of acts which we call trial-and-error, spontaneous, or random activity. The S's involved do not consistently elicit specific R's; the reactive tendencies

are weak or unformed, and the patterning of the series is variable. Random activity is in general, unorganized, and in ontogeny usually precedes the more integrated patterns of habitual or insightful behavior.

It should be noted that the end or consummatory response, the ultimate outcome of all four of our types of prolonged behavior sequences, is the same. Ingestion of food may follow the relatively uniform pattern of instinctive or habitual response, insightful behavior, or trial-and-error searching.

The evidence that there are sensitizing factors which integrate unit acts into stable patterns, these leading to biologically adaptive consequences, is very convincing. In other words, observation justifies and substantiates the concept of "drive behavior," which term covers all four of the above-mentioned categories of prolonged behavior sequences. In animal behavior, especially, the integrating factor can often be identified as one which promotes the biological welfare of the individual or species. It eventuates in consequences which provide near-optimum relations between the organism and its environment. That is to say, the organizing factor may appropriately be termed a biogenic or homeostatic drive. When the organism is operating under the influence of deprivation or hormones, a differential selectivity of behavior becomes manifest. It becomes hypersensitive to some stimuli, obtuse to others. The sensitizing factor intensifies responsiveness to some stimuli, reduces or inhibits responsiveness to others. It both focuses and filters the potential stimuli provided by the environment.

#### MECHANISMS OF THE DRIVE

We must now ask: what is the mechanism of the sensitizing factor of behavior determination? If we keep in mind the fact that some behavior, even in mammals, is largely independent of experience—the fact that the release of hormones into the blood-stream may differentially activate innately determined S-R's—we see that the question of how the drive influences habit-formation is subsidiary to this larger problem of differential sensitization in general. The problem of learning is extremely

important, whether we are dealing with man or with insects, but it must not obscure or divert us from the even more fundamental and more general problem of the organizing factor in all behavior.

It appears that many of the needs of the organism are in the first place physiological disequilibria; the animal requires oxygen or nutrients and it must get rid of wastes, extremes of temperature, and pain-producing agents. These organic needs must somehow be transmitted or "communicated" to the nervous system which controls behavior. In the nervous system, consequently, there must be organizations which correspond to the observed behavioral phenomena of differential sensitization. The nature of these neural organizations is still very obscure. They are often called "centers," thus implying that these neural organizers have a focal spatial location, occupying a specific and delimited area of the brain. Stellar (26) has recently marshalled the evidence supporting the currently favored idea that these centers are located in the hypothalamus. It seems just as possible and plausible that a "center" consists of a far-flung network of functionally interactive neurones which are distributed from the brain-stem or even cord to the cerebral cortex. However that may prove to be, the center must consist of a system or network of facilitating and inhibiting nerve elements. In the recent literature terms like "potentiation," "priming" and "pre-sensitization" have been used in conceptualizing the activity of nerve nets, circuits or centers. As Lashley has pointed out (14), the neurological problem is essentially the same whether we are dealing with sets, expectations, intentions, attitudes and attention as when we are considering "drive behavior." The neurophysiological mystery is essentially the same, and just as great, when we intend a certain effect and then "automatically" carry out the movements which bring about that result as when we spend a lifetime carrying out a major ambition. What is it, Lashley asks, that enables the musician to think of a Mozart concerto and then proceed to play it with no thought of the sequence of individual notes and chords? Or for the speaker to "have a thought" and then express it effortlessly in the proper words and correct grammar, sometimes in any

one of several languages? For the present we must de-tour around these questions.

A second, equally recondite problem is how physiological disequilibria activate the appropriate neural center. As you know, there are two hypotheses about this, one more plausible in some cases, the other one more applicable in other instances, but neither one adequately demonstrated. There is the theory of visceral sense organs or nerve endings stimulated by tensions or distensions of organs like the bladder or seminal vesicles. The stomach-contraction theory of hunger also belongs here. To explain the periodicity of either peripheral stimulation or of behavioral expression, the concept of central pace-makers (10) of "spontaneously" and rhythmically discharging nerve centers, or of the piling up of action-specific "energies" (Lorenz) is sometimes evoked. The second theory is of course that of centers which are differentially sensitive to the presence or absence of certain chemicals, nutrients or hormones in the bloodstream. A low blood-sugar level chemically activates the food-getting center, either directly or by affecting the centers which cause stomach contractions. Presence of certain steroids activate the centers which sensitize sexual responses.

## LEARNING

Whatever its mechanism, the effect of the sensitizing factor is to increase the probability of certain units acts occurring—that certain S's will be followed by certain R's—and furthermore that the particular concatenation of unit acts will be such as to eventuate in a certain end result. In an idealized instinctive sequence the S's which will be effective and the R's which occur to those S's, are all innately determined. According to Tinbergen's diagrammatic schema of the hierarchical organization of instincts (27), the only thing which can interfere with a smooth and straightforward execution of an ordered sequence of events is a perverse environment which does not provide the proper stimulus at the proper point in the series. Tinbergen de-emphasizes the point stressed by his teacher, Lorenz, that even in the lower animals

any prolonged sequence of behavior is composed largely of appetitive or, as we would say, preparatory behavior which is variable and often learned, and only in small part of innate releasing schemata and *Erbkoordinationen*. The flow of behavior is described by Lorenz as an interlacement of innate and learned responses.

The main problem of motivation with which American psychologists have concerned themselves is that of learning—especially the role of drive in causing instrumental learning. That the drive or sensitizing factor does influence habit formation is, I think, obvious enough. The problem of how it does so—whether by reinforcement of S-R sequences which are followed by drive-reduction; by merely instigating general activity which is terminated by attainment of the goal, the most recent and therefore the goal-attaining responses being the ones most likely to be repeated; or by any one of the many other mechanisms which have been proposed—this is an important problem which could well be the subject for another symposium. I wish to discuss only two points particularly relevant to the present discussion.

(a) You will recall that according to Lorenz (15) it is not the biological consequences of an activity, it is not homeostasis or the restoration of a physiological balance which the animal strives for and which causes learning. The goal of purposive behavior is, rather, the execution of an instinctive action or *Erbkoordination* which only incidentally produces a biologically significant result. The action-specific energy of an instinct promotes appetitive or searching behavior until the sensory pattern which releases the innate motor pattern is attained. Whether the consequences are good, bad or indifferent—that is, whether they fulfill any biological purpose—is irrelevant and unimportant. The animal does not “know” that it needs water or food—it only “wants to” drink or to eat. The acts of drinking or eating reinforce those preceding responses which led to the stimuli making drinking or eating possible. This notion, of course, is not entirely new; it is foreshadowed in some of the earlier but now disregarded theories of learning, such as the “confirmation,” “congruency” and “completeness of

response" theories of Hobhouse (11), Holmes (12) and Peterson (23), respectively. The idea is given support by experiments such as those of Hull (13) and Sheffield (25), showing that learning occurs when the animal merely eats, regardless of whether the eaten substance ever reaches the stomach or has any nutrient value. Of course it may still be that the sensory feed-back of eating movements or food in the mouth are the reinforcing agent.

(b) I would remind you that among animals at all levels of the phyletic scale there can be observed highly motivated, almost compulsive forms of behavior which have no relation to homeostasis in the usual sense of that term. The nest-building and brooding behavior of birds, the web-building activities of many spiders, and the care of the young by a great many different animals do not have any obvious effect on maintaining the physiological equilibrium of the individual engaging in these activities. And Professor Harlow (8) told you last year how the mere act of looking, of perceiving the environment, can and does promote the learning of habits which make such looking and perceiving possible. Butler's monkeys (6) formed stable discrimination habits—such as responding always to red instead of to green—when the only consequence of making the correct response was to be given an opportunity to look out of the confining box at the somewhat broader but still rather dull vista of an animal experiment room. (It is possible, of course, that the subjects were females—Peeping Tesses, perhaps—and that sight of the handsome experimenter rewarded one of the better known biogenic drives.) At any rate, it is in the interpretation of these findings that I differ with Dr. Harlow, and I think it will further the ultimate aims of our discussion to consider these differences.

#### THE BIOGENIC DRIVE TO EXPLORE, TO PERCEIVE, TO KNOW

Harlow listed several characteristics of *human* motivation, implying that these differentiated man from other

animals—with the partial exception of monkeys and apes. He said that man's motivation "may be independent of, or far detached from, the homeostatic drives."

I should like to raise the question of whether there can be any nonhomeostatic or nonbiogenic drives. These terms, the contrast or dichotomization of biogenic-nonbiogenic, imply two realms—one physical and one something else. It suggests that man can transcend the limitations of his body and has strivings unrelated to the functioning of his organs. Of course that is not exactly what Harlow meant, and he certainly is not alone in making the dichotomy. All that is meant, perhaps, is that some kinds of behavior make good biological sense and are relatively understandable, whereas other kinds do not fit the hypothetical framework that we have adopted; they force us either to introduce a category of rather mystically emergent "psychogenic" drives, or to make quite arbitrary and unsubstantiated assumptions about the derivation of secondary from primary drives. I do not deny the problem or its importance—drives do fall into two classes, one of which fits the concept of homeostasis as it was originally formulated by the physiologists (3, 7) and another one which does not fit that particular formulation. I do feel strongly, however, that the uncritical acceptance and widespread use of such contrasting terms as biogenic-psychogenic or homeostatic-nonhomeostatic perniciously introduces and perpetuates an unreal, misleading basis for the distinction.

The solution that I have come to is an extension or elaboration of one that was proposed long ago by Woodworth (30). I have already quoted his 1947 statement elsewhere (22) but it will bear repetition:

"The present thesis . . . is that perception is always driven by a direct, inherent motive which might be called the will to perceive. . . . To see, to hear—to see clearly, to hear distinctly—to make out what it is one is seeing or hearing—moment by moment, such concrete, immediate motives dominate the life of relation with the environment."

Just because this makes common sense is, I feel, inadequate grounds for rejecting it. In one fell swoop it explains a great deal of behavior—most of that which

we call exploration, curiosity and play, for instance—that resists other explanations. And anyone who has observed mammalian behavior, and for that matter behavior of so-called lower animals also, will realize how great a proportion of activity is devoted exclusively to keeping in touch with the environment, finding out what's going on, keeping informed, getting acquainted with a strange environment or with changes and new objects in a familiar one. Such behavior is particular conspicuous in young individuals, to whom almost everything is still new. The older, more experienced animal already "knows it all" and can afford to be blasé and to let his bored glance sweep only occasionally over the familiar scene. Some of you may recognize that the interpretation which I am here supporting is related to the biological or growth theory of play put forward in 1910 by Appleton (2) and the self-expression theory suggested in 1934 by Mitchell and Mason (17).

If, then, we accept the drive to perceive or to know as a basic and "primary" drive, can we also accept it as a "homeostatic" or "biogenic" drive? I think so. In the first place I do not think that as biologists we can accept a superbiological entity or force. Secondly, the nervous system is part of the body and as such it has homeostatic requirements comparable to those of other organs.

Among the requirements of all tissues is that they perform their normal functions. An unused muscle atrophies, and so does an unused gland. It is positively painful to deny any organ the exercise of its usual function. The desk-worker goes to considerable trouble and expense to get an outlet on the golf course for energies which are not needed or used in the service of other drives. Sensory deprivation is one of the most punishing aspects of penal confinement. The sense organs "want to" see and hear and feel just as much as the mouth or stomach or blood-stream "want to" eat or contract or maintain a certain nutrient balance. It is the function of the brain to perceive and to know. Rats will overcome the punishment of electric shock in order to explore a novel, stimulus-rich part of their environment (21). Montgomery (18) has shown that rats will master

a Y-maze with no incentive other than the opportunity to explore. Is it then unreasonable to postulate a primary drive—a drive for one of the main organs of the body, the brain, to perform its function of perceiving and knowing? The fact that the nervous system mediates the needs of other organs does not imply that it has no needs of its own. Every other organ has its own requirements; it makes demands on other parts of the body as well as contributing to the requirements of other organs. Why should the brain alone be completely the servant of the rest of the organism? As a matter of fact, when we think of ourselves, of man, do we not rather tend to make our ego—our feelings and sensations and thoughts—the boss, our main excuse for being? Do we not think of our hands and feet, our metabolic and digestive processes, as merely the instruments or means which make our behavior and mental functions possible? One extreme is probably as wrong as the other. The behaving individual is a cooperative enterprise, involving the coordinative functioning of its parts. Some parts may be more dispensable, replaceable, or repairable than others, but I doubt that any one can be considered superior to the rest. But if you insist that there is an autocrat, a little godlike part of the organism whose existence justifies all the other complex machinery of the body, then I shall insist that this is the brain and not the stomach.

The idea of a drive to perceive and to know may be stated more broadly: *capacity is its own motivation*. That is, capacity implies, or carries with it, the motivation for expression. The organism does anything that it can do. It is so constructed that if its bone, tendon, muscle and nervous structures permit it to walk or to fly, it *will* walk or fly. The mathematically gifted individual will tend to play around with figures and perhaps develop an algebra or calculus. The chimpanzee, not gifted in this direction, has so far shown no inclination to engage in play or other activities involving the number concept. This ape does, however, show considerable inclination to climb trees and to brachiate, a performance for which he has considerable talent, thanks to his bodily proportions and leverage systems. Contractile tissues illustrate the principle especially well; muscles which have not

been exercised otherwise will perform their potential function in a yawn or backward arching of the back. This thought may be some consolation to the long-winded professor whose own indulgence in vocal exercise is preventing similar drive-reduction on the part of his audience. From introspective evidence I should say that the drive to yawn or to stretch is at times very strong—almost irresistible.

You can see that we have here a certain unification of all drives and also a way of relating cognitive functions to motivation. It is obvious, of course, that there are limiting factors which operate to influence the general principle. The drive to rest, produced by fatigue, is one such limiting factor. A second one is that the upper, ultimate ranges of capacity not only provide a weak drive, but seem to operate against resistance; it may be that they are especially subject to fatigue. Thirdly, as the organism matures, habituation of channelization of behavior promotes repetitive acts and ways of thinking, limiting time and opportunity for expression of the full repertoire of capacities. Lastly, I would suggest that when we complain that our children *could* do better in their studies, that they are just not motivated to do their best, it may sometimes be that earlier failures to grasp the fundamentals of a given subject, say arithmetic, limit the capacity of the child to perform at a higher, more advanced level.

#### STRENGTH AND PERSISTENCE OF MOTIVATION

But let us return, now, to Harlow's characterization of human motivation. We have, I hope, disposed of the notion that biogenic and psychogenic drives are fundamentally different and that, except for monkeys and apes, the latter play little or no role in motivating animals. His second point is that "Man's motivation is extremely strong and persistent." I doubt that the motivation of animals is any less strong, or that strong motivation is exhibited only in connection with the so-called biogenic drives. The mother rat, or the mother bird defending its young even at the cost of its own life are familiar examples. Is there ever behavior more intense

than that of the cornered wild animal fighting for its existence? Or, for that matter, of the insect flying repeatedly at the electric light until it falls to the ground from exhaustion? The life of the guinea pig seems to be motivated 24 hours a day, from birth to death, by its hunger drive. Chimpanzees form friendships, and enmities, which last a lifetime. The drive of curiosity, they say, killed the cat. I should be inclined to add that boredom has perhaps killed even more.

But let us not be unfair to our eminent colleague from Wisconsin—he has a legitimate point. Although animals as high in the scale as chimpanzees—and this may be true of lower animals as well—do bear grudges and have long-lasting attitudes, it is true that their more violent emotional expressions are often evanescent. They may fly into a violent rage, and a few seconds later be perfectly calm, as if nothing had happened. In this respect they are like human children, and some of the insane. Furthermore it is undeniable that much human behavior is more devious and indirect than is that of any animal. Man often approaches his goals by extremely circuitous means, as contrasted to the more direct approach characteristic of most animal behavior. This certainly gives the appearance of greater persistence of motivation, but this appearance may be a function merely of man's capacity to see that the long way round may be the quickest—and most efficient—way to the goal. In other words, this seemingly greater motivational persistence is a function of his different, and as we say, superior, cognitive capacities. I doubt that the animal "gives up" any more easily, any quicker, than does man. It is merely that man's capacity for detours or *Umwege* is greater. The animal, with its more limited means of solving problems can only repeat the solutions of which it is capable, and if these are unsuccessful, to go hungry or die.

As to man's continuing "to carry out previously learned performances over long periods of time," the evidence indicates that in this respect animals are in no way inferior. A single experience with a distinctive noxious stimulus often evokes the learned avoiding reaction years or a lifetime later. In chimpanzees we know that a well-learned discrimination habit is retained for several

years—much longer than I can remember a phone number or the French equivalent for ham and eggs. Man remembers more because he learns more, but I doubt that he remembers what is learned longer than does the animal. As for the statement that in man the “demands of physiological drives may be . . . ignored during the course of these motivated performances” it is my guess that, in the course of writing a book or some other highly motivated and prolonged activity, even man takes time out to eat, drink, sleep, urinate—and so on.

The observation that most of “man’s complex learning is motivated by nonemotional or mildly pleasurable stimuli” also applies to animals. We found out long ago that starving a chimpanzee retarded his learning rate, rather than accelerating it. In a formal experiment Birch (4) found that the learning of tool-using problems was adversely affected by extreme hunger; it seemed that the animal was so preoccupied with the goal, namely food, that other elements in the situation, namely possible means to the goal, were overlooked. Conversely, I think it may be questioned whether man’s highest accomplishments are achieved so dispassionately. There may be few outward signs of emotion during the writing of a thesis or examination, but I suspect that such activities are often accompanied by much emotion and are, indeed, possible only under the pressure of intense motivation.

## PLAY AND EXPLORATION IN MAN AND ANIMALS

As for the comment that man attempts to solve problems which have “no apparent utility and may even produce personal pain or harm”—we have already seen that animals may suffer pain and death in the course of their struggles for an important goal. The phrase “problems which have no apparent utility” is of course the joker. Our children ask us “What’s the use of my taking Latin—it’s never going to be of any use to me.” But we parents think it does have utility. And what is the utility of so-called play behavior, in which animals as well as people indulge for long periods of time? I have already indicated the important function it may have in forming perceptions and giving the animal knowledge of the

world in which it lives. W. I. Welker of the Yerkes Laboratories has been conducting a study (29) in which chimpanzees are presented with various surfaces and objects—usually more than one at a time—for a period of 5 or 10 minutes. The initial approach of the animal to the new object is usually tentative—it gives a quick poke and then quickly withdraws. After the chimp has satisfied itself that the object is not dangerous, it begins to explore and investigate it more thoroughly. The more heterogeneous or variegated the object, the more attention it gets. A surface extravagantly decorated in many colors gets more response than an otherwise similar surface painted a uniform color. An object that moves, or one that produces an effect when touched, such as ringing a bell, elicits longer and more detailed manipulations than one which does not move or produce an effect. Interest, or attention or responsiveness—whatever you prefer to call it—gradually wanes. The number of contacts decreases within a session of 5 or 10 minutes and then increases again at the beginning of the next exposure to the same situation. There is, also, a session-to-session decrement of responsiveness. The animal has found out what it needs to know—whether the object is harmful or beneficial—and then it loses interest.

If the material and situation permits, the chimpanzee may tear or smash the object to fragments. Rarely if ever does it try to put the pieces together again. You may recall that Harlow's monkeys (9) would persistently disassemble a 5-part puzzle device, but practically never attempted to reassemble it. Destructive tendencies are prepotent over constructive ones, although these are seen occasionally in such activities as nest-building and stick-joining. In contrast to even the highest nonhuman primates, man's curiosity is more constructive and also, we might say, more "objective." I am inclined to explain this difference in part by man's greater capacity for perception, especially for perceiving relations. Since the world can mean more different things to him, he has more possible relations to explore; there are more potential relevancies to his goals and to the means for attaining them.

Where an animal does perceive a relation, as in the

monkey puzzle or even in a discrimination problem, the problem or activity often seems to provide its own motivation, as it frequently does in man. I have occasionally fed a chimpanzee all the banana it would eat and then brought it to a half-learned discrimination problem. At the end of the session, the animal had improved its score on the problem, but all the banana rewards were left uneaten. Dr. Schiller (24) gave chimpanzees two sticks which would be united by inserting one into a hole in the end of the other one. After a few days the animals could be observed industriously putting the sticks together and taking them apart again. There was no "profit" in this, no reduction in a so-called homeostatic drive, but they obviously enjoyed the activity. As matter of fact, when these same animals were later put into a situation where they needed a long stick in order to rake in a piece of apple which was on a platform, out of reach, for a long time they made no attempt to construct a long stick out of the two short ones. This was the case even though these animals had previous experience in raking in food with a single stick. To them, the fact that the joining activity resulted in a long stick either had not registered, they being intent on the joining process itself, or the relation perceived in one context was not applied to the demands of a different situation.

#### THE CONCEPTS OF STIMULUS AND DRIVE

Let us return now to the function of the drive or sensitizing factor in organizing unit acts into stable patterns. More specifically, let us raise the questions (a) of how many and what kinds of drives there are, and (b) whether the postulation of derived or secondary drives is necessary.

The usual definitions given of "stimulus" and "response" are circular: an S is something that produces a response, and a response is something that follows an S. Now there do seem to be some movements or responses of the organism which occur without an S in the usual meaning of that term; the *Leerlaufreaktion* and certain "spontaneous" and rhythmically occurring activities of the body would be examples. As far as I can see,

however, there is no way of defining an S without reference to a consequence, namely an R. If the response is not overt, observable, and measurable, then we infer that it is implicit or central; without such observation or inference, there is no point in speaking of a stimulus. Certainly mere exposure of the sensorium to environmental energies is not enough to insure that there has been an S. Everyday experience as well as experimental results show that there are many such exposures which are without any detectable effects on the organism. A chimpanzee, for instance, may learn a discrimination habit with multiple cues; later tests with these differentiating cues presented singly show that one of the cues is sufficient to produce errorless performance whereas another one, equally exposed to the sensorium and known to be easily discriminable by the subject, gives chance performance.

By definition, therefore, any S has a dynamic, energizing quality or attribute. This lends some justification to identifying "stimulus" and "drive." A light, or a movement in the periphery of the visual field, for instance, practically compels or forces an eye movement which brings the stimulus to the center of the field. The S literally drives us to make the response. Such use of the term "drive," however, deprives it of distinctiveness and therefore of its usefulness. Our earlier criteria of "drive" suggest that the term be applied where a sensitizing factor is effective over a period of time, influencing responsiveness to a number of stimuli and holding them together in a more or less predictable way. Neal Miller (16) has said that learned drives "have the same drive and cue properties as strong external stimuli." I would of course agree that a strong and persistent external stimulus, such as loud noise, sensitizes avoidance responses and may fixate, through learning, those which result in escape. With Dr. Brown, however, I would agree that the sensitizing function of the noise does not direct the response—that function is performed by other stimuli, possibly by the location of the sound. If one arranges it so that the sound is not localizable, then other features of the situation determine the direction, if any, of the response.

Miller's notion that the drive has cue properties is in line with the very old and still prevalent superstition, inherent in the very word "drive," that the organism is driven and guided to certain external goals, such as money or murder, by a mysterious force or homunculus who sits somewhere inside, preferably in the heart or in the brain. Brown, I think, has successfully laid that ghost. We have seen that responsiveness is a function of the threshold of a given S-R. When the threshold is low, as in a so-called reflex, the response is almost certain to occur, but not because there is some force, external to the SR, which "pushes" and directs it. The dynamics or "driving force" of the act is in the S-R itself. The drive is not an entity, steersman, helmsman or boss; it does not come into conflict with other bosses or drives, nor with the environment. It is merely the collective sensitization of a constellation of acts which are related in so far as they tend to produce a certain alternation in the organism or in one of its relationships to the environment.

### "ACQUIRED DRIVES"

Just why Miller (16), Mowrer (19), Brown (5) and many others have found it necessary to postulate secondary or learned drives is, I must confess, a great mystery to me. The so-called learnable or learned drive which has received the most attention is, of course, anxiety. It is supposed to derive somehow from pain, usually via fear. But no one has given a satisfactory or even plausible account of how pain is transmuted to fear, and fear to anxiety, nor have the differences between pain, fear and anxiety been clearly described. In ordinary, everyday meaning, pain is a sensation which innately elicits withdrawal responses. Fear and anxiety, on the other hand, are feeling states involving a strong component of anticipation or expectancy. These feeling states in themselves do not produce any directed responses. In fact, a frequent characteristic of anxiety is that there is nothing to withdraw from—the individual does not know what he is trying to escape. The presence of a directive stimulus—for instance, sight of the previously pain-producing

agent—actually tends to reduce anxiety, especially if the situation is such as to permit a directed and effective escape.

In this general area of discussion, the question of whether drives may be dichotomized into those which involve approach to a desired object or state of affairs, and those which involve avoidance of things unpleasant or painful, has been given considerable attention. In recent times, the idea that all drives are of the latter type has been in the ascendancy. The animal is motivated to reduce stimulation; learning is produced by drive-reduction. In order to maintain consistency on this score, the proponents of secondary drives are forced into some rather awkward contortions. In discussing the acquisitive drive or motivation to get money, for instance, Brown postulates that there is a learned tendency to be insecure—that is, to be anxious—in the absence of certain objects, namely money. However, the anxiety does not evoke acts which lead to the acquisition of money. To assume that would be to admit that a drive resulted and terminated in increase of stimulation, and that, of course, would be inconsistent with the idea that learning occurs only when the animal escaped from stimulation. So the explanation offered is that attainment of riches reduces insecurity by providing stimulation (money) the lack of which first produced the insecurity. At this point I detect a slight vertigo in myself which is not reduced when I try to figure out how the absence of stimulation produces a response, namely the learned drive, which then acts like a strong external stimulus which in turn elicits behavior which, in turn, is terminated by the onset of external stimulation, namely money. Just how the secondary-drive and drive-reduction people will go about explaining investigatory or exploratory behavior, whose effect certainly is to increase stimulation, I am not able to fathom. Perhaps they will again invoke anxiety, in this case produced by ignorance, or absence of knowledge about the environment. It does seem to me that whether we describe any instance of goal-directed behavior as approach or avoidance, as an increase, decrease or change of stimulation, is quite arbitrary, and that it is fruitless to try to make the distinc-

tion. Any statement in terms of approach or increase can be translated into one of avoidance or decrease, and vice versa.

Let us try to look at the problem in a more direct way. Any response tendency may be delayed, frustrated, inhibited or made difficult by one of several circumstances: (a) There may be environmentally imposed obstacles or barriers—there may be a fence between the animal and the food, for instance. (b) The physical or mental equipment of the organism may be inadequate to the task—it may not be able to run fast enough to catch the prey, or it may not be able to figure out how to attain the suspended banana. (c) There may be other, prepotent and incompatible response tendencies within the organism which conflict with its expression. Any such frustration or delay—whether of a so-called approach or avoidance tendency—produces tension and striving. The tension or anxiety shown by a mother chimpanzee when her baby is taken away is indistinguishable in quality and intensity from that exhibited when she herself is under threat of danger. Some stimuli innately elicit approach or avoidance responses, and when the performance or execution of such responses is prevented or delayed, excitement of some kind results. In animals this excitement is usually explicit and observable; in man it is often more internal, in which case we speak of anxiety. Relief, satisfaction, or drive-reduction, and cessation of the excited behavior, usually follow performance of the delayed act or the consequences—in terms of an altered organism-environment relationship—of that act.

In the course of habit formation or instrumental learning the animal acquires stable responses to stimuli which originally evoked either variable or consistently different responses. Our first and most fundamental basis for describing and classifying a response, I have said earlier, is whether it brings the animal closer or away from an object or source of external stimulation. All learned responses, therefore, have a goal-direction, they are to or away from something. The further refinements of response-description are not relevant or important in the present connection. The point here is that all the environmental objects and events concerned in these

learned responses have the character of goals—of positive or negative goals. These learned goals do not terminate the behavior, but are followed by further responses until the consummatory response and its goal are reached. In a prolonged habit-sequence, therefore, we have a series of sub-goals, a number of learned or acquired rewards and punishments. An object or condition attains its goal-property by virtue of being related, in experience, to an innately given sensitizing factor. There is, in other words, a series of signals, of positive and negative valences; and the connections between these signals and their responses are all sensitized by the same factor which sensitizes the final S-R. There is, then, no need to postulate new, learned drives or sensitizing factors over and beyond those innately provided.

Students of human motivation emphasize that a relatively small proportion of man's behavior is devoted to such activities as going to the restaurant or grocery store and eating the food we get there. Instead, as they rightly point out, we devote ourselves to other activities, including those which get us the money to buy groceries. Now the proponents of "learned drives" actually put the burden almost exclusively on just one such drive: fear or anxiety. The idea that most human behavior—including that involved in our jobs, professions, and recreations—is motivated by fear and insecurity is reminiscent of, and indeed may be a relic of the old notion that all life is a perpetual struggle for survival. I do not believe that what we know of animal and human behavior justifies this gloomy and nihilistic view that all behavior is but an escape from hunger and fear.

#### AUTONOMY OF THE UNIT ACT

Our rather one-sided preoccupation with the insistent demands placed on the organism to maintain those conditions which are essential for survival tends to obscure the fact that a large if not major share of the day's activities do not, in any obvious way, contribute to the basic necessities of life. This is true of man and animal alike. We therefore tend to assume, although we do not usually make the assumption explicit, that all behavior sub-

serves, and may be classified under, one of the major motivational systems. According to the ethnologists, such as Tinbergen, all behavior is organized under one of the major instincts or *Stimmungen*, which in turn brings into play its subinstincts or sensitizers which finally determine the detailed patterns of muscular response. The smallest act is a contributing part of the grand strategy determined by one of the instinctual chiefs of staff. If, as sometimes happens, a given act obviously does not "belong to" or subserve a function of the then commanding instinct, it is assumed that an act which really belongs to a different instinct has somehow slipped in. This is called a "displacement reaction." A private or a squad has gotten into the wrong company or regiment. Those things, of course, will happen.

We, who talk of drives rather than of instincts, tend to make a basic assumption, namely that habits, trial-and-error responses, and even the "spontaneous" behavior in an activity cage are all "energized" or "activated" by one of a limited number of drives. We expect behavior to be patterned or integrated, and to make biological sense; and so patterning and integration and biological utility are what we see.

And of course what we see is actually there—behavior in general is not chaotic; it is organized, and the organization comes from what we call drives or sensitizing factors. The fact that much behavior is so patterned as to realize major homeostatic goals does not mean, however, that all activities are thus organized into goal-directed series of units or acts. Observation of animals and people in "free," and even in highly "structured" situations indicates that much time and energy is taken up by brief, self-contained, often repetitive acts which are their own reason, which are autonomously motivated, and which are not to be interpreted as being small contributions to some remote, critically important aim. The organism scratches itself, stretches, shakes its head, grunts or chirps, looks around, examines an irrelevant item of the environment, picks its nose or its teeth, rocks back and forth, shifts its position, twists a paper clip out of shape, cracks its knuckles, doodles, looks at a picture on the wall, and so on *ad infinitum*. In the context of a

serious discussion a person may feel compelled to tell a joke, relate an anecdote, watch an airplane seen through the window, or concentrate on the peculiar shape of the speaker's nose. Some of these behaviors are the expression of innate or at least very basic propensities such as attending (responding) to the figure on a visual, auditory or tactual ground. Some classify as tics, stereotypes, or compulsions. Some are functions of what we call "the force of habit"—S-R sequences which may or may not have originated as acts subserving some larger purpose.

Among the characteristics of that which is an organism are anabolizing, katabolizing, reproducing—and responding. Any environmental energy or relation of energies which is a potential S tends to be an S, and therefore to evoke a response. By virtue of habituation or of innate predisposition, the nature of the R may be determinate and predictable. Or the polarity of the S may be weak, vacillating, indeterminate. The animal may approach, or avoid, in any one of several ways. The point is that just because the act does not further a major motive does not mean that it is unmotivated; just because it cannot be interpreted reasonably as a part-act or accessory of one of our basic drives does not imply that either it or its goal are unimportant to us or to the organism. No behavior is ever really accidental, incidental, or negligible; it always has a cause. And the examination of obscure causes may be more enlightening than that of perfectly obvious, and often incorrect "causes."

This line of thought was instigated, in myself, by the observation over long periods of time of captive chimpanzees (a) in relatively free situations—everyday cage behavior, we call it—and (b) in situations where sexually naive animals were paired, in male-female combinations during estrus, that is during periods when the female is in the physiological condition of being receptive to the male. I have already given a brief description of these "sex-observations" elsewhere (1). Here we had male-female pairs, ready for mating behavior in terms of age and hormones. The sex drive of the adult, experienced chimpanzee, male and female, is with few exceptions extremely strong. It calls forth all the symptoms of sexual desire as we know it in man, from great restlessness

and unwavering attention to the desired partner, to the loss of all interest in food. And under all other circumstances food and eating are among the very strongest motivating conditions for chimpanzees, any impressions to the contrary given by certain primatophiles notwithstanding. And yet, among these inexperienced pairs of animals, copulation was very, very slow in appearing. As a matter of fact, in most cases I finally had to use a sexually experienced chimpanzee as tutor for the naive animal of opposite sex. Only in this way could the young adult animal be initiated into the rites of a biologically essential function. The fact that copulation has to be learned—that it is not a structurally determined bit of behavior which occurs under appropriate hormonal conditions as in most other mammals—this fact is interesting but somewhat irrelevant to the present discussion.

What is pertinent here is the observation that these animals, under the influence of a strong sensitizing agent ordinarily leading to a very specific goal, displayed a wide variety of *other* activities, most of which had no relation whatsoever to that goal. Some of those activities are among those I listed earlier—scratchings, hootings, pacings and so on. Others involved social interactions: wrestling, play-fighting, and grooming. What is the motivation of grooming (“flea-picking”)? It somewhat resembles human “petting,” but more often than not it follows copulation, rather than preceding it. And it may often be seen in female-female pairs who are not homosexual in any meaningful, Kinseyan sense of the word. My answer is that chimpanzees like to—are motivated to—groom, and so they groom. We don’t have to look for some other, superordinated drive to explain its motivation.

It is clear that most unit acts do or may occur in the context or service of several different drives and also when no drive in the usual sense is operative at all. The sleeping infant, replete with all the milk it can hold, makes sucking movements. Incidentally, some adult chimpanzees do this when they are awake—like thumb-sucking, it seems to be an outlet for boredom, when there is nothing better to do. Locomotion is a good example in point—the young chimp may run to get the banana,

to get away from a tormenter, or just to run for the sake of running. The insane talk, sometimes, whether they have a listener or not, and their words and sentence structure are the same as ours. The act of talking is pleasurable in and for itself. There is a drive to talk, just to talk, even among those we do not classify as insane, and regardless of whether the talking conveys information, sells the washing machine, or impresses the boss into giving a raise.

### RECAPITULATION

Let me now summarize briefly the main points of my discussion.

1. The dynamics of behavior are in the inherent attribute or quality of all living matter to be sensitive; that is, to be responsive to stimuli. They do not reside in any force or entity that is outside of or superordinated to that quality.
2. "Drive" is an inferred influence of bodily states on response thresholds, particularly on fairly specific concatenations of S-R thresholds which are related, through consummatory acts or their consequences, to those bodily states.
3. The effect of drives is to organize longer sequences of unit acts into instinctive, habitual, or insightful patterns of response. The neural organizations underlying drive behavior, and the mechanisms of activating those neural organizations, are imperfectly understood. One of the principal ways by which drives integrate behavior is by making instrumental learning possible.
4. The need of organs to function is a bodily state which expresses itself in drive behavior. Capacity is its own motivation. A function or capacity of the sense organs and brain is to perceive and to know, and this is one of the more important drives of all organisms.
5. All drives are biogenic and all are primary or innately determined. The complications of hierarchical motivational organization derive from the acquisition of new signs and subgoals.

6. Many unit acts are autonomously motivated, being relatively independent of the major sensitizing factors which are called drives.

### References

1. ALLEE, W. C., NISSEN, H. W., and NIMKOFF, M. F. A re-examination of the concept of instinct. *Psychol. Rev.*, 1953, 60, 287-297.
2. APPLETON, L. E. *A Comparative Study of the Play Activities of Adult Savages and Civilized Children*. Chicago: Univ. Chicago Press, 1910.
3. BERNARD, C. *Leçons sur les propriétés physiologiques et les alterations pathologiques des liquides de l'organisme*. Paris: Ballière, 1859, Volumes I and II.
4. BIRCH, H. G. The role of motivational factors in insightful problem-solving. *J. comp. Psychol.*, 1945, 38, 295-317.
5. BROWN, J. S. Problems presented by the concept of acquired drives. In *Current Theory and Research in Motivation: A Symposium*. Lincoln: Univ. of Nebraska Press, 1953, 1-21.
6. BUTLER, R. A. Discrimination learning by rhesus monkeys to visual-exploration motivation. *J. comp. physiol. Psychol.*, 1953, 46, 95-98.
7. CANNON, W. B. *The Wisdom of the Body*. New York: Norton, 1932.
8. HARLOW, H. F. Motivation as a factor in the acquisition of new responses. In *Current Theory and Research in Motivation: A Symposium*. Lincoln: Univ. of Nebraska Press, 1953, 24-49.
9. HARLOW, H. F., HARLOW, M. K., and MEYER, D. R. Learning motivated by a manipulation drive. *J. exp. Psychol.*, 1950, 40, 228-234.
10. HOAGLAND, H. *Pacemakers in Relation to Aspects of Behavior*. New York: Macmillan, 1935.
11. HOBHOUSE, L. T. *Mind in Evolution*. New York: Macmillan, 1901.
12. HOLMES, S. J. *The Evolution of Animal Intelligence*. New York: Holt, 1911.
13. HULL, C. L., LIVINGSTON, J. R., ROUSE, R. O., and BARKER, A. N. True, sham, and esophageal feeding as reinforcements. *J. comp. physiol. Psychol.*, 1951, 44, 236-245.
14. LASHLEY, K. S. the problem of serial order in behavior. In *The Hixon Symposium, Cerebral Mechanisms in Behavior* (L. A. Jeffress, Ed.). New York: Wiley, 1951, 112-146.

15. LORENZ, K. Z. Über die Bildung des Instinktbegriffes. *Die Naturwissenschaften*, 1937, 25, 289-300, 307-318, 324-331.
16. MILLER, N. Learnable drives and rewards. Chapter 13 in *Handbook of Experimental Psychology* (S. S. Stevens, Ed.). New York: Wiley, 1951.
17. MITCHELL, E. D. and MASON, B. S. *The Theory of Play*. New York: Barnes, 1934.
18. MONTGOMERY, K. C. The role of the exploratory drive in learning. *J. comp. physiol. Psychol.*, 1954, 47, 60-64.
19. MOWRER, O. H. A stimulus-response analysis of anxiety and its role as a reinforcing agent. *Psychol. Rev.*, 1939, 46, 553-556.
20. NEWCOMB, T. M. Motivation in social behavior. In *Current Theory and Research in Motivation: A Symposium*. Lincoln: Univ. of Nebraska Press, 1953, 139-161.
21. NISSEN, H. W. A study of exploratory behavior in the white rat by means of the obstruction method. *J. genetic Psychol.*, 1930, 37, 361-376.
22. NISSEN, H. W. Phylogenetic comparison. Chapter 11 in *Handbook of Experimental Psychology* (S. S. Stevens, Ed.). New York: Wiley, 1951.
23. PETERSON, J. Completeness of response as an explanation principle in learning. *Psychol. Rev.*, 1916, 23, 153-162.
24. SCHILLER, P. H. Innate constituents of complex responses in primates. *Psychol. Rev.*, 1952, 59, 177-191.
25. SHEFFIELD, F. D. and ROBY, T. B. Reward value of a non-nutritive sweet taste. *J. comp. physiol. Psychol.*, 1950, 43, 471-481.
26. STELLAR, E. The physiology of motivation. *Psychol. Rev.*, 1954, 61, 5-22.
27. TINBERGEN, N. *The Study of Instinct*. London: Oxford Univ. Press, 1951.
28. WARDEN, C. J., JENKINS, T. N., and WARNER, L. H. *Comparative Psychology*, Vol. II, 1940, Plants and Invertebrates; Vol. III, 1936, Vertebrates. New York: Ronald Press.
29. WELKER, W. I. Determinants of exploration and play in chimpanzees. Unpublished dissertation, Univ. Chicago, 1954.
30. WOODWORTH, R. S. Reënforcement of perception. *Amer. J. Psychol.*, 1947, 60, 119-124.

## A Behavior Primacy Theory of Motivation

ROBERT S. WOODWORTH

### A BEHAVIOR-PRIMACY THEORY OF MOTIVATION

The main contention of this book—seemingly a perfectly obvious and innocent view—is that behavior consists in active give and take between the organism and the objective environment. This interrelationship may be called “dealing with the environment.” To deal with the environment the organism must do more than merely receive stimuli and make muscular responses; it must use the stimuli as indicators of objects in space, and use the muscles for movements adapted to the objects; that is exactly what the organism does. A dog’s ears receive a certain shrill tone; he turns his head in the direction of the source of the tone which is issuing from the lips of his master; immediately the dog’s leg muscles begin a series of contractions which carry him over the ground to his master. Evidently the dog has done much more than receive an auditory stimulus and make a series of muscular contractions. The whole performance is geared to the objective environment—to the master as a known object, to the location of the master, to the more-or-less level ground and its unevenness, and to other demands of the situation.

In later chapters we try to show that the organism perceives and learns such objective facts as the size, shape, location and other characteristics of objects. Here we are making the claim that this direction of receptive and motor activity toward the environment is the funda-

---

Excerpted from Robert S. Woodworth, *Dynamics of Behavior*. New York: Holt-Rinehart-Winston, 1958.

mental tendency of animal and human behavior and that it is the all-pervasive primary motivation of behavior. That is what we mean by a behavior-primacy theory of motivation.

What we mean by calling the objective tendency of behavior "all-pervasive" can be brought out by comparing playful incidental activities, such as doodling, with any well-developed purposive course of action. Purposive behavior has a definite goal or end, with means adopted to reach the goal. Incidental playful behavior is not aimed at any remote goal, but it deals with the immediate environment and produces objective results. It is not random muscular activity, for it uses stimuli as indicators of objects such as the paper and pencil, and it produces immediate results which are geared to the objects. From his careful observation of the behavior of caged chimpanzees, Nissen (1954, pp. 313 ff.)<sup>1</sup> has reached the following conclusions:

Our rather onesided preoccupation with the insistent demands placed on the organism to maintain those conditions which are essential for survival tends to obscure the fact that a large if not a major share of the day's activities do not, in any obvious way, contribute to the basic necessities of life. This is true of man and animal alike. . . . Observation of animals and people in "free," and even in highly "structured" situations indicates that much time . . . is taken up by brief, self-contained, often repetitive acts which are their own reason, which are autonomously motivated, and which are not to be interpreted as being small contributions to some remote, critically important aim.

This pervasive tendency to keep in touch with the environment and to be doing something with objects is clearly present when the "object" is another person. The conversation may not be aimed at any particular conclusion, but there is a constant give and take between the two people. The "goal" is simply to be engaged in social behavior.

Instead of saying that all this incidental behavior is

<sup>1</sup> H. W. Nissen, The nature of the drive as innate determinant of behavioral organization. In *Nebraska Symposium on Motivation*, edited by Marshall R. Jones. Lincoln: Univ. Nebraska Press, 1954, 281-321.

secondarily motivated by the organic needs or other great motives, we insist that the incidental behavior represents the primary drive to deal with the environment and that large-scale purposive activities are based on this primary drive. In order to motivate food seeking, the hunger drive has to break into the on-going behavior and give it a special direction. Young boys often resist this intrusion of mealtime into their absorbing play.

### *Prediction from the behavior-primacy theory*

Our complaint regarding the need-primacy theory, as represented either by Freud or by Hull, was that it failed to predict the motives and interests of human adults or the play interests of children. Even the play of the higher animals could not be predicted from the organic needs alone. To obtain greater predictability we should have to add something closer to behavior. What we propose is that the behavioral *capacities* of any species must be added. From the bird's capacity to fly we can predict that the bird will fly and apparently like to fly or at least prefer flying to walking. If the bird is the domestic fowl (*gallus domesticus*) with slight capacity for flight, or for swimming, it will be content to walk on the ground for the most part, but if it is a pigeon or a duck, it will take to the air or to the water. Can we say that any such locomotion is merely mechanism, having no intrinsic motivation but driven by some extrinsic need? What seems to be true, rather, is that the young creature makes considerable progress in mastering its mode of locomotion before it begins to use this skill in hunting for food. Obviously, we cannot credit the young creature with so much foresight that it will exercise its flying capacity in order to be ready to fly in search of food later. The flying is done for its own sake at first, and only later in the service of organic needs. If we can say that any behavioral capacity has its own intrinsic motivation, our theory has much greater power of prediction.

A capacity-primacy theory of motivation has been proposed more than once. Does not the capacity for muscular activity demand muscular exercise, and the capacity

for seeing demand the ocular activities of looking and focusing? The drive to actualize one's capacities (Goldstein, 1940)<sup>2</sup> would accordingly be an important source of a great variety of human interests. This theory could be improved, so it seems to us, if we recognize that each capacity is a capacity for dealing in some way with the environment. Seeing is not simply getting visual stimuli; it quickly develops into a seeing of objects in space. Muscular activity quickly develops into a reaching for objects or locomotion toward them. Even the human capacity for recall memory and ideational thinking develops into a high-level method of dealing with the environment in the solution of problems and in planning for the future. The capacities develop, not as purely intraorganic processes, but in the "life of relation" with the environment. The motivation to deal with the environment is primary and includes the various ways of doing so which are provided by the capacities.

There is an interesting parallelism between the theory presented here and the "holistic" theory of Kurt Goldstein (1939,<sup>3</sup> 1947).<sup>4</sup> The organism, he insists, necessarily functions as a whole and its motivation is unitary. Its "basic drive, the only drive by which the life of the organism is determined" is the "tendency to actualize its nature, to actualize 'itself' . . . the drive of self-actualization." But since the organism has various potentialities or capacities, self-actualization includes a variety of activities which actualize its capacities. "We can say, an organism is governed by the tendency to actualize, as much as possible, its individual capacities, its 'nature' in the world" (1939, pp. 196-198). What the present theory says is that the inclusive drive is the tendency to deal with the environment, and that the capacities are capacities for dealing with the environment in various ways. Actualizing a capacity, accordingly, amounts to learning

<sup>2</sup> K. Goldstein, *Human Nature in the Light of Psychopathology*. Cambridge, Mass.: Harvard Univ. Press, 1940.

<sup>3</sup> K. Goldstein, *The Organism: a Holistic Approach to Biology Derived from Pathological Data in Man*. New York: American Book, 1939.

<sup>4</sup> K. Goldstein, Organismic approach to the problem of motivation. *Trans., N.Y. Acad. Sci.*, 1947, 9, 218-230.

how to deal with some aspect of the environment. Goldstein seems to imply that the actualization of any capacity is strongly ego-involved, whereas we believe that O's interest can be objective and concentrated on the materials dealt with and the activity of dealing with them.

From the human child's vocal and linguistic capacities we can predict a good deal of playful behavior. Mere exercise of the vocal muscles would probably have little reward value to the child, but by combining the activity of mouth and larynx with a blast of air from the lungs he produces a variety of audible sounds which evidently are rewarding, for he produces them over and over with no extrinsic motivation. He engages in vocal give and take with favored friends and gradually learns more definite ways of communicating with them vocally. On occasion he communicates his need for food, but he acquires his speaking vocabulary in other ways for the most part. Of the hundreds of words that the child picks up before the age of three, very few are concerned with bodily needs. Most of them designate persons, animals and things that interest him or activities in which he participates. Vocalization and speech begin as autonomous play activities rather than mechanisms driven by the necessities of life.

### *Combination of organic need and the behavioral tendency*

We are not pretending that the organic needs are derived from the tendency to deal with the environment. The organic needs are autonomous. But the behavior that is enlisted in the service of an organic need has its own rewards apart from the reduction of a need. The act of sucking appears to have rewards since not only does the infant demand more of it than is necessary (Jersild, 1954),<sup>5</sup> but older people take pleasure in sucking cider through a straw or smoke through a cigarette. Adults have a sense of mastery in raising water from a cistern by a hand suction pump. In fact, they are exert-

<sup>5</sup> A. T. Jersild, Emotional development, in Carmichael, L. Ed. *Manual of Child Psychology*, 2nd ed., edited by L. Carmichael. New York: Wiley, 1954, 833-917, esp. p. 844.

ing some control over nature by taking an active part in a physical process (usually without understanding it). Again, when the baby arrives at the biting and chewing stage, he devotes himself assiduously to these ways of mastering objects; and adults are known to chew gum vigorously long after they have extracted all its taste. The point is that behavior which ministers to an organic need is still behavior dealing with the environment and has its own immediate behavioral achievements, consummations, and reinforcements.

Some such immediate external rewards are well known, but they are usually regarded as secondary and not primary reinforcements. They may be both at once, for there is no reason why a stimulus that has some primary reward value should not also become a token or signal of important reinforcement to follow. The click of the pellet-delivering machine in the Skinner box is such a token and becomes a secondary reinforcement for bar pressing, as demonstrated by the extinction test. Now even a naive rat that has never obtained food pellets by pressing the bar and obtains none during the experiment will press the bar once in a while, i.e., at a relatively slow rate called the "operant level," so manifesting his general activity level or exploratory tendency. These foodless bar pressings are usually supposed to be getting no reinforcement. But they are sure to obtain some sensory feedback from the feel and movement of the bar. The feedback is increased if the apparatus delivers a buzz or a dim light each time the bar is pressed; and the operant level is then raised considerably. The buzz or light must be accepted as a *primary reinforcement* of the bar pressing, since neither of them has ever been followed by food in the individual animal's experience; neither of them has had any chance to become a secondary reinforcer (Kish, 1955).<sup>6</sup>

Another form of apparatus (Figure 1) offers the animal a choice between a platform that moves and clicks when stepped on and another platform that gives much

<sup>6</sup>G. B. Kish, Learning when the onset of illumination is used as reinforcing stimulus. *J. Comp. Physiol. Ps.*, 1955, 48, 261-264.

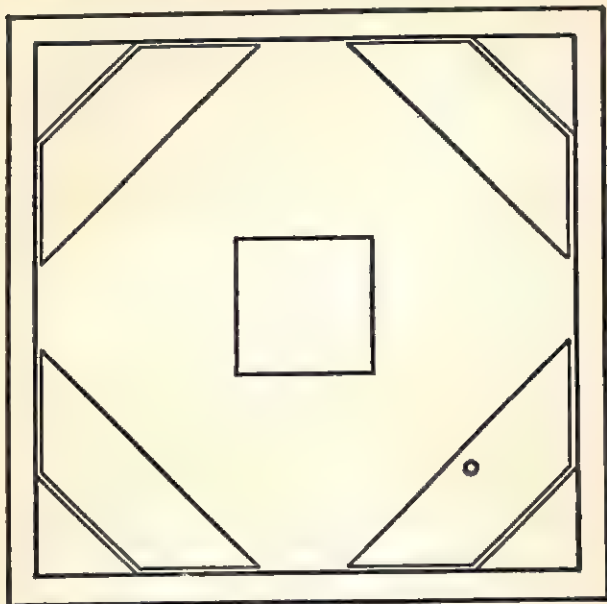


Fig. 1. Floor of box used for testing incentives with mice as subjects. (After Antonitis & Kish, 1955.)

less sensory feedback; the platform that moves and clicks is greatly preferred. The experimenters, Kish & Antonitis (1956),<sup>7</sup> reach this conclusion:

Apparently, this stimulation, coming as a direct result of a response to the platform, was acting in a reinforcing manner and strengthening the behavior directed toward the region of the platform. . . . At the time the experiment was planned, there appeared to be no *a priori* reason for considering this class of stimuli, which have usually been considered neutral, as reinforcing. The results indicate, however, that the sensory consequences of a particular behavioral act may, under the proper conditions, reinforce that behavioral act.

This type of reinforcement, though quite out of line with Hull's insistence on need reduction as the primary

<sup>7</sup> G. B. Kish and J. J. Antonitis, Unconditioned operant behavior in two homozygous strains of mice. *J. Genet. Ps.*, 1956, 88, 121-129.

reinforcement, did not escape his keen incidental observation. We have mentioned his 1934 experiment on speed of locomotion toward a goal. He equipped a long straight runway with a series of light hanging doors of cardboard which the rat pushed open one after another as he ran toward the food box. He noticed that these doors "seemed not to deter the animals in the least; indeed, observation even suggests that the rat, at least, may have a definite liking for such contacts."

*Significance of these examples of primary behavioral motives and reinforcements*

A critic might be inclined to question the importance of the examples just given. He might say that such sporadic and superficial interests in environmental happenings and results accomplished have no bearing on the practical work or deep motives of human beings. These, he might insist, depend on the basic needs of the organism, or of the psyche. We offer the counterclaim that our examples have a double significance.

1. They serve as crucial experiments, disproving the claim of the need-primacy theory to universality. No one denies that organic needs are factors in motivation, but the need-primacy theory asserts that all primary motives are organic needs or at least basic needs of the organism. The exploratory drive may perhaps be accepted as a basic need of the organism, a need to find out what is present in the environment. But the cases cited go beyond the exploratory drive. Exploration of a cage reveals a platform in each corner, for example, but the platform that makes a noise when stepped on is stepped on most often afterwards. This preference goes beyond the goal of discovery or of novelty, for the response to the preferred object is repeated time after time. Exploration reveals things that are "good" or "bad" or indifferent. The things that are good then receive continued positive response, the bad things are avoided, and the indifferent things disregarded. But among the good things are not only such as appeal to the organic needs but also things that can be made to move or make a noise or emit a light or odor. A child on a visit, if given the run of the house,

will locate not only the pantry and bathroom but also a surprising number of things that are interesting because he can do something with them. You cannot expect him to keep busy most of the time attending to his organic needs.

2. The examples point the way to a host of objects and activities which are attractive and challenging to human children or adults and which provide their own direct reinforcements. We have mentioned the child's vocalization and acquisition of speech, and we might have added his learning to read and the reinforcements he obtains when he recognizes the words and still more when he gets the story from the printed pages. We mentioned in the preceding chapter the skills and teamwork of adult games with their direct rewards for successful action.

Human beings have capacities for dealing with the environment in many ways that go far beyond simple manipulation and locomotion, and the culture provides opportunities for the use of these capacities. It provides tools and materials to work with, models to imitate, praise or dispraise for the results accomplished. What is the motivation of a child trying to hammer in a nail? Is it to win applause and avoid criticism? Or is not his primary goal to drive the nail straight without banging his fingers, and is not his primary reinforcer the well-driven nail? It would seem that the person with capacity for construction is motivated to construct and obtains his most direct reinforcement from the result achieved, so that the engineer who has designed and put up a fine bridge is proud of the bridge itself rather than of the money and applause received. The same can be said of any inventor; there may be large economic rewards in the offing, but he likes the material he is dealing with and his primary reward is the successful solution of a problem.

Musical capacity shows itself early, given a favorable environment. The potential musician is attracted to the piano and does much more than just pound on the keys. He does not need an adult to stand over him continually to tell him when he is striking the right notes; he can tell himself from the sound. His reinforcements come from the musical effects.

A child who makes a convincing drawing of a face,

even of some particular person's face, wins applause. But where does he look while drawing? Not at the source of admiration but at his subject and at his drawing. The applause encourages him to keep on and make other drawings, but while he is engaged in any one drawing his motivation and reinforcements are concentrated right there.

### *The source of human interests*

We have been attempting to suggest that the distinctively human interests in construction, invention, and art—and also in mathematics, science, and governmental and industrial administration—have their source, not in the organic needs, but in the human capacity to deal with the environment. They arise from the combination of human capacity with the opportunities afforded by the natural and cultural environment. The evidence for this conclusion comes partly from the way these interests emerge and develop in childhood, and partly from the marked *absorption* of the scientist or artist in the task that occupies him at any time. Mathematicians, inventors, novelists, composers are reported as doing their most creative work in periods, short or prolonged, of intense absorption in the present task. Any intrusion of extraneous economic or social motives would be distracting and defeating. Even ego-involvement retires before the intensity of task-involvement. And there is this also to be said of the ego-involvement of the creative worker. He may have high and strong personal aspirations, but they are concentrated on his line of work. He aspires to become, not an all-round good fellow, but a great painter, or composer, or chemist. His ego-interest is tied to his work and when he is engaged in some piece of work, his ego-interest has to be pushed aside for complete concentration on his task.

*Predictive success of the behavior-primacy theory.* If the need-primacy theory of motivation could predict the emergence of distinctively human interests, the achievement would be greatly to its credit, because it would predict so much from so little. But need primacy does not predict any greater variety of motives for humans

than for animals, since the needs are the same for both. Of course, the need-primacy would have the right to assume human capacities for dealing with the environment; it could then predict the development in human societies of elaborate ways of dealing with the environment to meet the organic needs. But there would be no corresponding development of motives and interests. All the elaborate methods of meeting the needs would belong in the sphere of mechanism, not of drive.

The behavior-primacy theory regards the tendency to deal with the environment as a primary drive, and indeed as *the* primary drive in behavior. The various capacities for dealing with the environment afford outlets for the general behavior drive and give it different forms—given the necessary environmental opportunities. So the manifold human interests are predictable from the combination. It is no great credit to the behavior-primacy theory to predict so much, since the base from which it predicts is so broad. Can it be accused of assuming all that it wishes to predict? Not quite; it assumes the general drive to deal with the environment, the capacities for dealing with the environment in various ways, and the opportunities for such dealing afforded by the environment. What it predicts is that the interaction of a capacity with the environment will generate a specific interest. It predicts that an individual who engages in a task will, unless distracted, find himself absorbed in that task, interested in it and motivated to bring it to completion.

Perhaps the behavior-primacy theory, even though no great intellectual achievement, will have practical value in education, where the need-primacy theory certainly appears to be useless. Even in psychotherapy, it may be, the person who is disturbed by conflict of deep personal motives could be led to take a normal interest in some way of dealing with the environment for which he has the capacity and the opportunity. This practice has been followed on a common-sense basis even by therapists who espouse a need-primacy theory inconsistent with the practice.

## Stimulus-Response Theory of Drive

W. K. ESTES

*Indiana University*

Since the publication of Hull's *Principles of Behavior* (23) in 1943, his two-factor theory of drive has moved into a steadily more dominant role in the literature of behavior theory. For a number of years, a principal motif was that of testing the two-factor conception. More recently, and especially in Spence's *Behavior Theory and Conditioning* (39), we find the validity of this conception no longer treated as a live issue. Attention is turned instead to such matters of detail as determining the arithmetical rule by which habit and drive interact.

Reasons for the hegemony of Hull's conception are not hard to find. Preceding formulations, insofar as they were specific at all, had viewed drive either as a particular type of stimulus (Guthrie [17]) or as a vaguely defined energizing function (46; 49, Ch. 2, 4, 5). By combining both stimulus and energizing factors into a single theory, Hull automatically enabled himself to explain far more phenomena than his nearest competitor. At the same time, Hull departed from custom even more radically by making his concepts reasonably definite and specific. This latter feature of Hull's formulation led directly to the parametric studies (8, 14, 21, 26, 31, 37, 45, 47, 48) which have done much to bring some semblance of order and structure out of a diffuse and amorphous research area.

I am sure I need not linger over the detailed manner in which Hull recast the well-weathered distinction be-

---

Reprinted from *The Nebraska Symposium* edited by Marshall R. Jones. Lincoln: Univ. Nebraska Press, 1958, with permission of the author and publishers.

tween learning and performance. His construct  $E$ , excitatory potential, is in turn a joint function of constructs  $H$  and  $D$ , representing strength of habit and strength of drive. The construct  $D$  is defined as a general energizing factor which combines multiplicatively with  $H$  in the equation  $E = D \times H$ , familiar by now to all readers of the *Journal of Experimental Psychology*. The magnitude of  $D$  increases with increasing deprivation of food, water, and the like, and with intensity of such stimuli as electric shocks. Increases in  $D$  are assumed to generate corresponding increases in excitatory potential of *all* habits in the animal's repertoire, regardless of the previous learning history. If stimuli for two or more habits are present simultaneously, then increases in  $D$ , owing to the multiplicative property, are assumed to increase the differences among the various  $E$ 's so that the highest  $H$  becomes more likely to win out over its competitors; thus increases in drive lead to stereotypy of performance. The second factor in Hull's drive theory, drive stimulus ( $S_D$ ), refers to stimuli, e.g., those caused by stomach contractions or a dry throat, which vary in intensity with deprivation time, and which presumably have the same properties as any other stimuli.

Since both aspects of drive are defined in terms of unobservable states or events, it seems prudent to be especially cautious and critical in appraising the empirical evidence for them. It is obvious enough that there are variations in behavior, even systematic variations, that are not attributable to variation in observable stimulus conditions; consequently we must expect some reference to unobservables in a theory of behavior. Prevailing conceptions of drive, especially Hull's, provide some explanation for many otherwise mysterious phenomena, and they have instigated much research. But because the concepts of primary drive and drive stimulus have been useful and inspirational, it becomes all the more likely that if either aspect of current drive theory should be seriously in error, it would constitute a serious blockade in the way of further progress.

With these considerations in mind, let us briefly review the evidence for the two drive factors.

(a) *Drive stimulus*. In some cases the term *drive re-*

fers specifically to an observable stimulus, e.g., an electric shock, a bright light, an intense noise, or to the immediate effects of such a stimulus. Concerning these instances, I am inclined to agree with a recent reviewer that "if these were all we had, it seems doubtful that the drive concept would ever have been invented."<sup>1</sup> What needs justification is using the same term to denote an inferred state or condition of the organism which is not known to be correlated with any such stimulus. In the case of hunger or thirst, there are associated interoceptive stimuli which can be observed by special techniques; but direct observation has neither established just how these stimuli vary with deprivation nor determined whether there are characteristic stimuli associated with all of the deprivational conditions we call drives. Thus any assumption that drive stimuli play a significant role in the determination of observed behaviors must rest primarily upon indirect evidence.

Two sources of indirect evidence are particularly relevant to this problem. The experiments of Hull (22) and Leeper (27), apparently indicating that rats can form discriminations based on different deprivational conditions (hunger and thirst), were one of the principal bases for Hull's conception of drive stimuli. Those early experiments were not adequately controlled in all respects by modern standards; however a long series of studies with progressively more refined controls (4, 20, 24, 1, 2) has built what appears to be an airtight case for the existence of discriminable cues associated with different deprivational conditions and with different degrees of deprivation. Our inferences from the studies of drive discrimination are fortified by the results of recent experiments on "irrelevant drives" which demonstrate that variation of food or water deprivation can affect learned responses exactly as does variation in intensity of a conditioned stimulus. Eninger (9), for example, has shown that resistance to extinction of a response trained by escape from shock is reduced by any change in the deprivational conditions (and by inference the drive

<sup>1</sup> K. MacCorquodale in *Annual Review of Psychology*, 1955, 6, p 30.

stimuli) that prevailed during training; complementarily, Wickens, Hall, and Reid (44) and Levine (28) have shown that reversal of a discrimination is retarded by the continued presence of an "irrelevant" deprivational condition (and by inference the continued presence of drive stimuli to which the formerly correct response has become conditioned).

It is important to be clear just what can and what cannot be concluded from this accumulation of indirect evidence. I see no basis for reasonable doubt concerning the generalization that in any given experimental situation, discriminably different patterns of stimulation can be produced by the operations of satiation and deprivation.

Available data do not, however, suffice to apportion the drive-associated stimulation between interoceptive and proprioceptive sources. In fact, we cannot even rule out exteroceptive sources. Some writers (6, 19) have hypothesized that increased deprivation of food or water simply lowers the thresholds for external stimuli. If this hypothesis were true, drive discriminations might still be possible, but they would be specific to the particular environmental situations in which training occurred. If a discrimination between hunger and thirst, say, could be shown to exhibit no transfer from one experimental situation to another, the threshold hypothesis would have to be taken seriously, but I know of no data bearing on this possibility. In any event, throughout the remainder of this paper I shall use the term *drive stimuli* to denote any stimuli whose probabilities of occurrence vary as a function of deprivation time, with no commitment as to the physiological locus of the stimulation.

(b) *Energizing factor*. The generally positive correlations between deprivation time (for food) and "spontaneous activity" (measured by records from a running wheel or a tambour-mounted cage) have long been a principal support of the concept of drive as an energizer. More analytical experimentation seems to have waited patiently for this rather vague concept to crystalize into the more definite form of Hull's *D* factor. From his formulation, Hull predicted that resistance to extinction

of an instrumental response would be an increasing function of the degree of deprivation prevailing during extinction; and several studies conducted to test this prediction have yielded generally confirmatory evidence. When, for example, hungry rats are trained to make an instrumental response for food reinforcement, and extinction is carried out at varying degrees of food deprivation in different groups, the number of responses to extinction proves to be an increasing function of deprivation time over the interval from zero up to the training deprivation (7, 21, 31, 37). In Spence's recent review, even greater weight seems to be given the results of Webb (43) and Brandauer (5), who found that if extinction after such training is conducted at different levels of water deprivation while the animals are satiated for food, frequency of extinction responding is an increasing function of water deprivation. Spence's interpretation is that the  $D$  factor associated with the "irrelevant" thirst drive increases the excitatory strength of the learned habit even when the training drive has been eliminated by satiation. For quantitative evidence concerning the multiplicative property of  $D$ , Spence (39) has appealed to the family of curves obtained when a measure of response strength is plotted against number of reinforcements with deprivation time as a parameter. If  $D$  combined additively with habit strength to generate excitatory strength, the curves of the family would be parallel, higher values of  $D$  simply displacing the curve further up the ordinate. If  $D$  combined multiplicatively with habit strength, the curves would diverge from a common origin to different asymptotes. Considering the empirical curve families of this sort that have been reported, one must conclude that there is very little support for the former alternative; gross deviations from parallelism are observed in almost all cases. Several studies, notably those of Perin (31), Williams (45), Ramond (33), and Ketchel (cited in [39]) have yielded clearly divergent curves, and Spence has taken these results as confirming the idea that  $D$  is a multiplier of habit strength.

The empirical findings mentioned in this brief review are but a sample of a large and heterogeneous array

which, on the whole, lines up behind the two-factor theory. The obvious conclusion would seem to be that the direction in which drive theory will develop is clearly marked, that the problems remaining for further research are merely those of working out variations on a well-established theme and filling in quantitative details,

But what if the obvious conclusion is wrong? Consider for a moment the possibility that the two-factor theory is basically unsound despite the tidy situation in which Spence's book seems to have left it. Should there be any basic weakness, can we safely assume that it will soon show up if we simply relax in the role of politely interested spectators while the busy workers of Hullian persuasion continue plotting curves and revamping functions? Perhaps so. But is there not some danger that once we have drifted into the habit of seeing our data through two-factor spectacles, we will simply respond to empirical warning signals by making slight changes in this or that assumption about the quantitative properties of *D*, thus forestalling indefinitely any serious attack on the ground plan of the theory?

We trust that experimental facts will lead us toward adequate theory, but they can do their part only if we permit them to speak out. If they are to tell us that a theory is unsound, we must be prepared to receive the message. One form of preparation is to assemble evidence unfavorable to the theory as carefully as we do evidence that supports it. A second is to try as hard to construct alternative theories as we do to prop up the one temporarily in favor.

Looking through Spence's review, supplemented by a sampling of current experimental journals, we find no single fact which could be taken to refute decisively the *D* construct, but we do find some which are of a character to raise doubts about it.

a. Recent studies have shown that the positive relation between activity and hunger can be strongly modified by variation in conditions of reinforcement (15, 34).

b. The facilitative effects of increased deprivation upon performance of an instrumental response during both acquisition and extinction come and go through a series of apparently ample experiments (29, 31, 40, 41) in a manner

which hardly suggests the operation of a simple energizing factor.

c. In Cotton's carefully controlled study of runway behavior (8), an orderly functional relation between running time and deprivation time, superficially most harmonious with *D* theory, virtually disappeared when running times were computed only for trials on which no clearly observable competing response occurred.

d. Experiments by Grice and Davis (16), Kendler (25), Verplanck and Hayes (42), and others upon interactions between thirst and hunger yield a complex pattern of interrelationships that is increasingly difficult to reconcile with the notion of a general energizing factor.

e. As Spence has noted (39), it is not unknown for families of curves representing response strength as a function of reinforcements with deprivation time as a parameter to display a distinct tendency toward convergence as trials continue—a most aberrant trend from the standpoint of *D* theory.

Finding that two-factor drive theory is not entirely adequate to handle all known facts in its intended area of application is not in itself a particularly newsworthy accomplishment. As much can be shown almost any day for almost any theory. What is interesting is that the pattern of discrepancies suggests a possible locus for constructive theoretical activity. In the literature, both experimental and theoretical, the almost universal tendency has been to account for as many relationships as possible in terms of drive as an energizer, holding the notion of drive stimulus in reserve. The latter is brought into the picture only when absolutely necessary to handle some otherwise refractory phenomenon. But of the two factors, that of drive stimulus is the one most nearly forced upon us by experimental facts. Why, then, would it not be reasonable to try reversing the coin—to account for as many relationships as possible in terms of the apparently indispensable concept of drive stimulus, leaving the notion of a general energizing factor to be brought in only if it really turns out to be necessary?

One's immediate thought may be that "this has been tried before." But has it? Guthrie (17) has long been identified with a stimulus conception of drive, but

Guthrie's views have never been developed in sufficient detail to be tested against specific experimental data. Even in Hull's formulation, drive stimulus has been the stepchild, employed only at a qualitative level while the *D* factor received the benefits of provisional quantification and formal incorporation into the equations defining excitatory potential.

### THEORY OF DRIVE-STIMULI

The remainder of this paper will be devoted to evening up the score. My objective shall be to do as thorough a job of developing the concept of drive stimulus as Hull and Spence have done for the energizing factor.

#### *Assumptions*

1. First of all, I assume that drive stimuli are simply stimuli, with no special properties whatsoever. Thus I shall be applying to a particular class of experiments the general stimulus theory developed in preceding papers by myself, Burke, and others (10, 11, 12, 13, 38). I shall not recapitulate previous papers here, but I shall review methods and assumptions as occasion arises to apply them in particular contexts. From this viewpoint, the essential characteristic differentiating experiments on drive from other classes of experiments is that in the former some of the important sources of stimulation are not directly observable. In order to reduce a very large task to conceivably manageable proportions, I shall deal almost exclusively with experiments upon hunger and thirst as determiners of instrumental response strength (in mammals), and within this still large class of experiments primarily with the parametric studies that have demonstrated reasonably clearcut functional relationships between response measures and degree of food or water deprivation.

2. In order to apply general stimulus theory to experiments on drive, we require explicit assumptions concerning the different sources of stimulation operative in an experimental situation. The four sources I shall distinguish are illustrated schematically in Fig. 1. The four

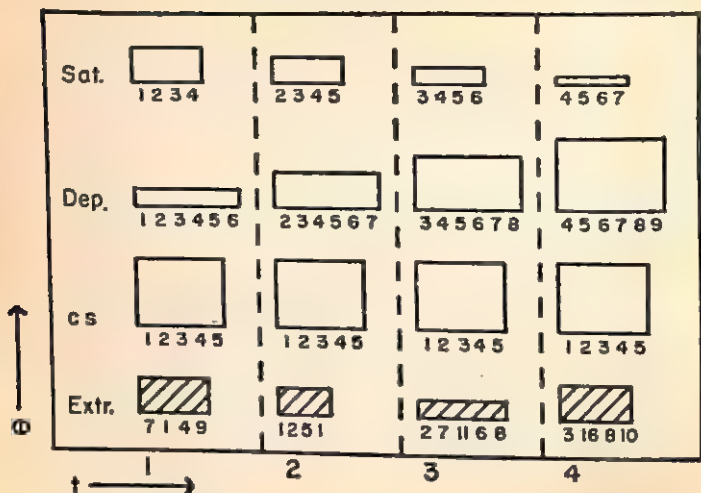


Fig. 1. Schematic representation of the four classes of stimuli assumed to be operative in any experimental situation. The classification is based on the relationship between deprivation time ( $t$ ) and stimulus sampling probabilities ( $\theta$ ).

columns in the figure represent an experimental situation at four deprivation times.<sup>2</sup> The row labelled CS represents experimentally controlled signals, discriminative cues, or the like, and other apparatus cues whose sampling probabilities can be assumed constant from trial to trial. The row labelled Extr. represents extraneous stimuli; these are randomized with respect to both time and experimental treatments by standard experimental designs. The row labelled Sat. represents cues, including for example those produced by distention of the stomach, whose probabilities of occurrence decrease with depriva-

<sup>2</sup> In the next few sections I shall use the terms "deprivation" and "satiation" only with reference to some one experimentally defined drive, usually hunger; only in connection with experiments on drive interaction, the last topic to be considered, will we have occasion to consider deprivation of both food and water concurrently.

tion time. Finally, the row labelled *Dep.* represents cues whose probabilities of occurrence increase with deprivation time.

The horizontal dimension of each rectangle in Fig. 1 represents the number of cues (stimulus elements) in the set, while the vertical dimension represents the average sampling probability of the elements. The area of the rectangle represents what we shall term the *weight* of the stimulus set; the weight is a parameter which can be evaluated experimentally and which may be taken to signify the relative importance of the given stimulus set in determining behavior.

3. Our only general assumptions concerning weights of the various stimulus sets are as follows. (a) The weight of the CS set is constant over deprivation time. (b) The weight of the Extraneous set varies randomly, but the average weight is constant over deprivation time. (c) The weight of the Satiation set decreases with deprivation time; for simplicity in calculations, I shall take the function to be linear, as shown in the figure. (d) The weight of the Deprivation set increases with deprivation time; again for simplicity I have taken the function to be linear.<sup>8</sup> (e) The combined weight of Satiation and Deprivation sets together increases with deprivation time.

4. The numerals under the various rectangles in Fig. 1 signify individual cues and are intended to illustrate my assumptions concerning overlap of the sets available for sampling at different deprivation times. In the case of the CS, exactly the same set of elements is assumed available at all deprivation times. The subsets of Extraneous cues represent random samples from a population large enough so that particular individual cues

\* Considering everything we know about the elementary drives, it seems unlikely to me that any single function will be found to hold generally for either Satiation or Deprivation cues; presumably the functional form can be determined empirically for any given set of conditions, if it should seem worth the trouble; for purposes of deriving qualitative and semi-quantitative predictions we shall simply have to hope that the assumption of "linearity over the greater portion of the range of the independent variable" will serve here as it has in so many other contexts.

will rarely recur on different trials or periods of an experiment. For Satiation cues, and similarly for Deprivation cues, we assume a sequence of partially overlapping sets, the proportion of overlap between any two sets in either sequence decreasing linearly with the time difference as illustrated in the figure. Thus as time passes following a period of eating or drinking, some of the Satiation cues produced by the consummatory activity drop out and are replaced by others, the latter being a consequence of digestion and other bodily activities that follow food or water intake with some delay; similarly the detailed makeup of the set of effective Deprivation cues is assumed to change progressively with time. The psychological significance of these assumptions is that of the cues which become conditioned to a response during training at a particular deprivation time, progressively more will be replaced by unconditioned cues as we test at deprivation times further from the training value.

5. Probability of any response is assumed to be determined by all cues present at the time of testing. More specifically, probability is assumed equal to the weight of all cues connected (i.e., conditioned, associated) to the given response divided by the weight of all cues present. In Fig. 1, and others to follow, I have indicated cues connected to the reference response by white areas and cues connected to competing responses by crosshatched areas. This figure might be taken to represent an experiment in which the reference response has received extensive training at all deprivation times. Thus all of the CS, Satiation, and Deprivation sets are represented by white areas. Presumably some Extraneous cues would also have become conditioned to the response, but since these rarely recur, the Extraneous sets are nevertheless represented by crosshatched areas. Now the probability of the reference response at any one deprivation time would be given by the area of the CS, Sat., and Dep. rectangles divided by the total area of all four rectangles.

### *General implications of the theory*

It will be apparent from Fig. 1 that in any one training situation, the effect of drive stimuli upon the asymptotic level of performance can be regarded as an amplifi-

cation of the effects of the CS. In standard simple learning situations, conditions of reinforcement are such that drive cues and CS cues will become conditioned to the same responses; under these circumstances, the CS-plus-drive cues will more heavily outweigh any unconditioned extraneous cues present than would the CS alone, thus yielding a higher response probability. The magnitude of this amplifying effect will depend upon a number of conditions. Some measures of performance are more vulnerable than others to the effects of extraneous cues. When rate measures are taken, as in a Skinner box, or response time measures, as in a runway, competing responses evoked by extraneous cues must almost invariably depress the measure of performance; but when relative frequency measures are taken, extraneous cues may have little effect. When, for example, a rat in a T-maze stops to scratch its head or explore a vagrant odor, the running time for the trial will necessarily be increased, but the choice of left vs. right alley will probably be affected, if at all, only upon trials when an extraneous cue disturbs the animal precisely at the choice point. Thus asymptotic rate of bar-pressing in a Skinner box and asymptotic running time in a runway or maze should be expected to depend strongly upon level of deprivation over a considerable range of deprivation times, while an asymptotic frequency measure in a two-choice situation should depend only weakly upon level of deprivation.<sup>4</sup>

If training in an instrumental situation were given at several different levels of deprivation, then one would expect, on grounds either of intuition or of general experience, that asymptotic performance would be an increasing function of deprivation time. Considering the successive columns in Fig. 1, we can readily see that this outcome is predicted by the theory; for the total white area increases as we go from left to right while the crosshatched area fluctuates around a constant value. However, we also note that if extraneous cues could be completely eliminated, the theory would no longer pre-

<sup>4</sup>This conclusion should be qualified if deprivation times become so low that response to the reinforcing stimulus is appreciably affected, a complication that will be ignored in the present paper.

dict an increase in asymptotic response probability with deprivation time. The white area would still increase, but since there would be no crosshatched area to be outweighed, response probability would be constant from column to column.

It is probably impossible to eliminate all extraneous cues, partly because some of them are internal in origin (e.g., itches, sneezes, coughs). However, Cotton, in a doctoral research done at Indiana University a few years ago, has done the next best thing by demonstrating the effects of removing from a set of runway data all trials on which behavioral evidence indicated the presence of appreciable extraneous stimulation. Cotton trained rats on his runway at each of a series of deprivation times and then tested asymptotic running times at all deprivations. He first plotted raw mean response times, then he eliminated from the protocols all trials on which observable competing responses (face-washing, exploring cracks, etc.) occurred and replotted mean response times for the remaining trials, presumably relatively free of the effects of extraneous cues. Predictions computed from the present theory for this experiment are illustrated in Fig. 2. In the upper panel are shown theoretical response probabilities for the two conditions, and in the lower panel theoretical response times.<sup>5</sup> Those who have read Cotton's paper (8) or who have seen his empirical functions reproduced in Spence's book will recognize the close correspondence between our predictions and Cotton's findings.

One assumption that appears tacitly in the various graphical and computational examples of this paper should be mentioned explicitly. Namely, I have assumed throughout that all drive cues (as well as the CS) are unconnected to the reference response at the beginning of an experiment and become connected only as a result of learning that may occur during the experiment. The assumption is made here because it simplifies calcula-

<sup>5</sup> From earlier work on statistical learning theory (10) it is known that mean response time should be approximately equal to the reciprocal of probability.

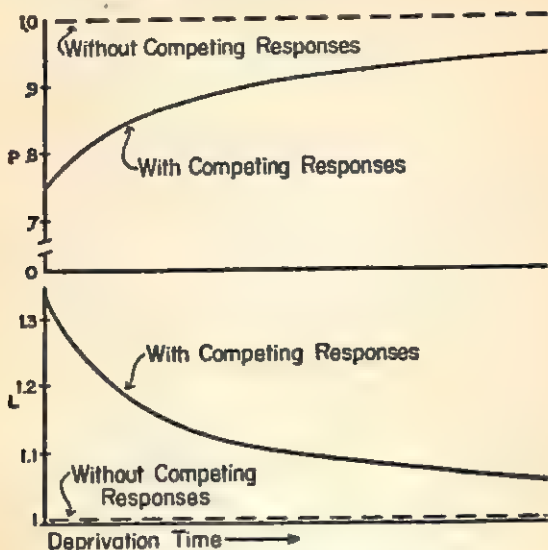


Fig. 2. Theoretical curves for Cotton's experiment.

tions in most instances, because considering the various alternatives in each instance would unduly complicate the exposition, and because in the particular kinds of experiments considered it probably is not far from the true state of affairs. However, we should recognize that as a result of previous learning, in or out of a particular experimental situation, the population of drive cues may be preponderantly connected to the reference response previous to an experiment in some instances and preponderantly connected to competing responses in others. In any particular experimental application of the theory, one would naturally attempt to make the most reasonable assumption concerning the initial state of the various kinds of cues. Once the assumptions concerning initial conditions have been made and confirmed experimentally for any given situation, then they will be treated as a fixed part of the theory while other aspects are being tested.

*Theoretical account of experiments hitherto taken as evidence for D factor*

1. *Activity level.* Apparently one important basis for Hull's assumption of an energizing factor was the positive correlation normally observed between deprivation time and such measures of activity as operant level in a Skinner-type situation, activation of a running wheel or tambour cage when no obvious source of reinforcement is associated with responding, and asymptotic response rates or response times in instrumental learning situations. According to the present theory, however, there is no universal or unmodifiable relation between activity and deprivation time. To the extent that behaviors which are tapped by a given measure of activity have become conditioned to drive stimuli, either during or previous to an experiment, a positive correlation between activity and deprivation is predictable by the reasoning sketched in the preceding section.<sup>6</sup> Even in these cases, the detailed nature of the predicted relationship differs for the two kinds of theory. On the assumption of a *D* factor, a high degree of deprivation should generate a greater excitatory potential than a low degree of deprivation, and for any particular measure of excitatory potential the difference between high and low deprivation groups should be constant over the period during which activity is measured. (A constant proportionality should obtain if habit strength varies over the period.) According to the stimulus theory, a higher degree of deprivation produces a higher total weight of drive stimuli which in turn tends to eliminate interruptions of the measured performance; at a low deprivation, pauses are more likely, because extraneous cues have greater relative weight, but between pauses the measure of response rate or speed should be the same as for a higher deprivation.

2. *Learning curves at different deprivations.* According to the stimulus theory, there are two distinct sources of differences between learning curves obtained at different

<sup>6</sup> I do not mean to rule out the possibility of innate connections between drive stimuli and certain behaviors, but I do assume that, at least for mammalian forms, any such connections are modifiable by learning.

degrees of deprivation. The first, already discussed, is the greater weight of the set of drive cues at higher deprivations. Since the weight differential leads to different asymptotes, at least when rate or speed measures are used, this factor taken alone would lead to a divergence of the curve family exactly as does the  $D$  factor. However, this factor does not operate alone. Referring back to Fig. 1, we recall that sampling probabilities of drive cues are higher, on the average, at higher deprivations. Consequently, conditioning of drive cues will proceed more rapidly the higher the deprivation. Since this second factor operates only during the pre-asymptotic phase of learning, its effect is to produce a greater divergence of the curve family early in the learning series than would be generated by the weight differential alone. Taking the two aspects together, we can foresee that the predicted relationship between pairs of curves obtained at different deprivations is one of divergence followed by partial convergence, as shown in Fig. 3. When

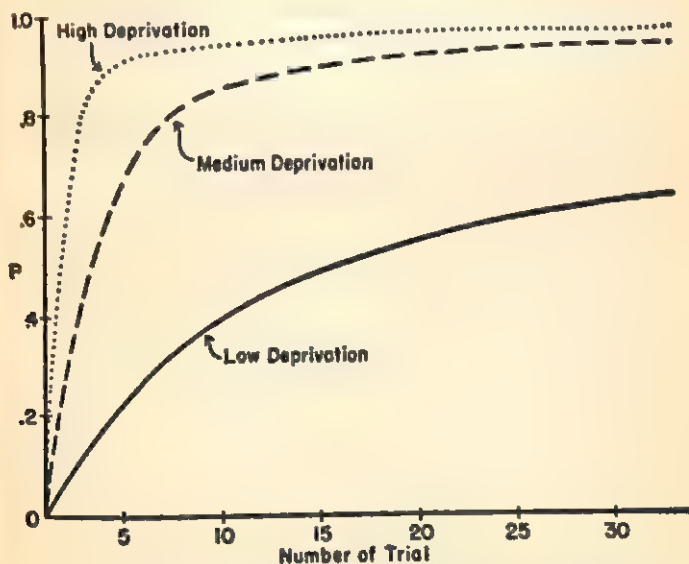


Fig. 3. Learning curves with deprivation time as a parameter.

we are dealing with a pair of curves both obtained at relatively high deprivations, as the upper two curves in the figure, or with curves for a frequency measure which is relatively insensitive to degree of deprivation, we must predict a divergence followed by convergence to nearly equal asymptotes. When we are dealing with curves obtained at widely separated deprivations (one of which must be near the low end of the continuum), and especially if a rate or speed measure is used, the predicted picture is one of wide divergence followed by a relatively small amount of convergence.

From this analysis it appears that amount of training must be an important parameter of all studies in which measures of performance at different deprivations are compared. Generally we should expect to find maximum differences at some intermediate degree of training and smaller differences, or in some instances none, after very small or very large amounts of training. Certainly consistency of results cannot be expected among different experiments in which relationships between performance and deprivation time are tested unless measures are taken at the same point in training for all of the experiments.

A technical consideration which must be mentioned briefly is that curves plotted in terms of responses to extinction following different numbers of reinforcements, as in the well-known studies of Perin (31) and Williams (45), are not expected to show quite the same pattern as curves plotted in terms of rate or response speed. It has been shown in earlier studies of statistical learning theory (10) that if probability of a response at the beginning of extinction is equal to  $p$ , the total number of responses occurring during extinction should be approximately proportional to  $\log(1/[1-p])$ . So long as  $p$  values are fairly small, this quantity is almost linearly related to  $p$ , and curve families for responses to extinction should be very similar to those for the other measures considered. But when  $p$  values approach unity, as they are often found to do after a sufficient number of reinforcements in simple learning situations, the log quantity diverges; under these circumstances, two curves obtained at different deprivations will be expected to resemble

those of Fig. 3 in their early portions but to show no convergence as the number of reinforced trials becomes large.

3. *Habit strength as a function of deprivation during acquisition and extinction.* This has been one of the favorite problems for investigators of drive in relation to learning, although by no means one of the easiest. According to Hull's formulation (23), variation in *D* should not influence degree of learning, defined in terms of habit strength, but only performance. Operationally, this means that on the basis of the *D* factor alone, performance during acquisition should depend on level of drive; performance during extinction should depend on level of drive during extinction; but performance during extinction should not depend on level of drive during acquisition. Hull's position was apparently supported by data (14) collected with the experimental design suggested by these predictions, namely one in which groups of animals receive acquisition at different deprivations and then all receive extinction at the same deprivation. The difficulty with this design, aside from the fact that it has not yielded consistent results, is that even within Hull's theory the *D* factor does not act alone. Drive stimuli cannot be eliminated, and within this design obviously some groups will have larger changes in stimulus conditions than others between acquisition and extinction. With the increasingly widespread use of analysis of variance methods during the postwar period, the obvious answer has been a factorial design in which the group receiving acquisition at each drive level is broken down into subgroups, one to receive extinction at each level. The recent popularity of this factorial design must be attributable to its methodological neatness, for the pattern of results yielded by it has been cloudy to say the least. Considering the main effects, extinction performance has usually been found significantly related to extinction drive, but in a few cases (40, 41) the relationship has failed to appear. On the other hand most studies have found no significant relation between extinction performance and acquisition drive, but in a very recent study by Lewis and Cotton (28) significant positive relations were demonstrated between extinction per-

formance and both independent variables. If the various individual studies have any reliability at all, they set a stiff task indeed for a theory to deal with. We must account for a normally positive but occasionally missing correlation between extinction responding and extinction drive, for a normally insignificant but occasionally positive correlation between extinction responding and acquisition drive, for the significant interaction effects sometimes (48) but not always (29) found in the factorial studies. It will be of interest now to see how close our stimulus theory can come to doing all this without benefit of special ad hoc assumptions.

The basis for predictions from the stimulus theory are illustrated in Fig. 4. In the upper panel we see the theoretical result of training at a low drive and testing at a high one. Changing the level of deprivation between training and test has the effect of dropping out some of the drive cues that had become conditioned during

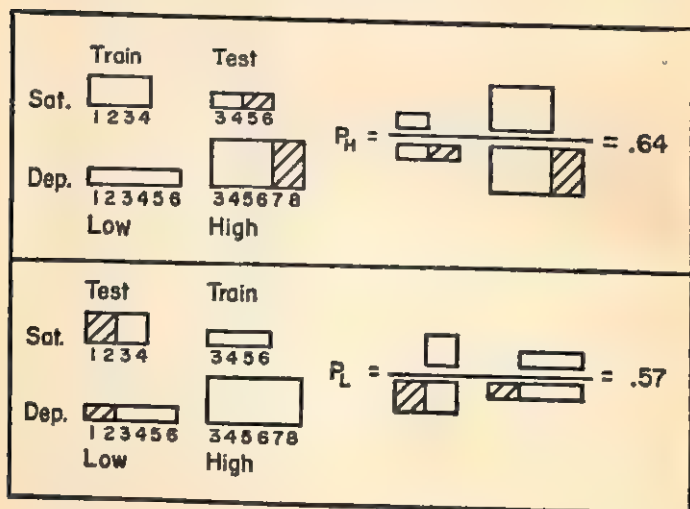


Fig. 4. Schema illustrating theoretical basis for predictions concerning generalization from training at one level of deprivation to a test at a higher (upper panel) or lower (lower panel) level of deprivation.

training and replacing them with new, presumably unconditioned, ones; response probability on the test (measured, say, by response speed at the beginning of extinction) is equal to the weight of the common cues divided by the total weight of cues present on the test period. (For simplicity, I have omitted CS and Extraneous cues from this figure.) In the lower panel we see the theoretical result of training at a high drive and testing at a low one. We note that although we began with the same response probability at the end of acquisition in both cases, probability on the test is higher when the shift is from low to high deprivation than when it is from high to low deprivation. This asymmetry is a necessary consequence of the stimulus theory if, as I have indicated in the figure, the proportionate overlap between the lower and higher Deprivation sets is equal to or greater than that between the lower and higher Satiation sets; I have tentatively assumed that this relation holds generally and have used it in all computational examples shown in this paper.

Choosing parameter values that satisfy the assumptions illustrated in Fig. 4, but without so far as I know any important special properties, I have computed theoretical predictions for a hypothetical factorial experiment in which both training (to asymptote) and extinction test are conducted at four different levels of deprivation. The numerical results are shown in Table 1. The cell entries may be interpreted as response probabilities, or reciprocal latencies, at the onset of extinction under the various combinations of conditions. It may be instructive to compare the principal trends displayed by the theoretical values of Table 1 with those of empirical values shown in Table 2. The latter are probability estimates (reciprocal latencies) from a study by Yamaguchi (48).

The asymmetry of generalization to higher as compared to lower deprivations shows up clearly in both the theoretical and the empirical data, and is greater the further the pair of cells considered falls from the main diagonal. Running down the main diagonal we have a series of groups each trained and tested at the same drive, and in both tables probability is a monotonic increasing function of deprivation. If we hold test depriva-

TABLE 1 Factorial Experiment: Theoretical Data

Training		Test Deprivation			
Deprivation	1	2	3	4	Av.
1	.75	.85	.78	.69	.77
2	.68	.95	.87	.78	.82
3	.60	.85	.97	.88	.82
4	.52	.76	.87	.98	.78
Av.	.64	.85	.87	.83	

TABLE 2 Factorial Experiment: Yamaguchi's Data

Training		Test Deprivation			
Deprivation	3	24	48	72	Av.
3	.92	.89	.95	.79	.89
24	.42	.99	1.00	.84	.81
48	.10	.88	.99	.82	.70
72	.06	.88	.89	1.00	.71
Av.	.38	.91	.96	.86	

tion fixed at the highest value, then running down the fourth column of each table, we see that test probability increases with training deprivation. If we hold test deprivation fixed at the lowest value, then running down the first column of either table, we see that probability decreases monotonically with training deprivation. In the second and third columns, the curve of test probability vs. training deprivation passes through a maximum at an intermediate deprivation.

To examine the effects of varying the test deprivation for a fixed condition of training, we need only run across the rows of these tables. For the highest training deprivation, the sequence of row values increases monotonically, and rather steeply, from left to right. For all other training deprivations, the row values increase to a maximum and then decrease.

If data such as these were subjected to an analysis of variance, the main effects would be represented by variation among row means and column means. Examining these, we note that the theory and data agree on two principal points: first, the main effects attributable to variation in test deprivation are considerably larger than those attributable to training deprivation; second, both main effects, although more especially that of training deprivation, could easily go undetected, even in a very substantial experiment, if the lowest deprivations used were not very near zero. If only the three highest deprivations were used, neither the hypothetical experiment nor Yamaguchi's actual experiment would show any trace of an "energizing" effect of deprivation when analyzed as a factorial design. On the question of whether interaction effects should be readily detectable in such a design, no definite statements can be made. If the animals used in an experiment were sufficiently homogeneous, a large interaction effect could be expected; if not, the curves of response probability vs. test deprivation for different animals might have their maxima at different points along any given row of the table, and interaction effects would be obscured. Thus one important conclusion that we can draw from this analysis is purely methodological: we cannot expect to unravel the effects of acquisition and extinction drives by the mechanical application of analysis of variance designs. A conclusion of more immediate psychological import is that the pattern of empirical relationships revealed by Yamaguchi's study could quite well have arisen from the action of stimulus variables alone and thus cannot be construed as evidence for a D factor.

Several elaborate attempts have been made to determine the detailed function relating response strength to deprivation time after training at a single deprivation (usually approximately 24 hours). The functional form prescribed by the stimulus theory is illustrated in Fig. 5. When probability or response speed is the dependent variable, the curve is negatively accelerated from the lowest deprivation up to the training value, rising steeply at first, then almost linearly over most of the range. When the dependent variable is number of responses

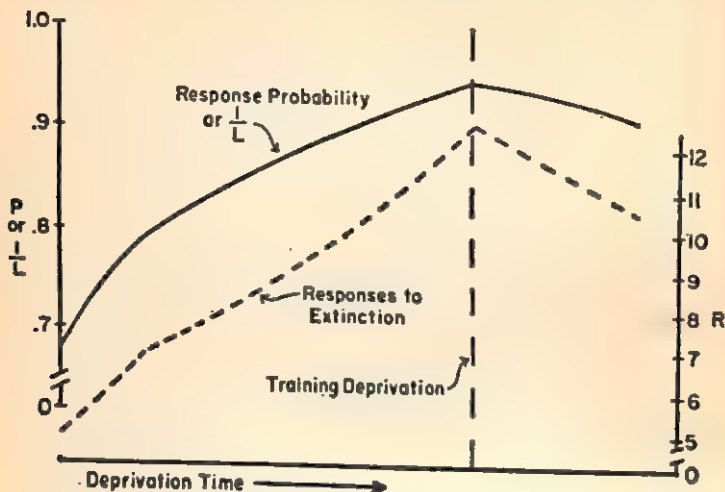


Fig. 5. Tests at various deprivations after training at a single deprivation.

to extinction, however, the curve becomes positively accelerated as the training deprivation is approached (assuming that response probability on the training deprivation is relatively high) for reasons we have discussed earlier. These predictions fare quite well when checked against available data. With reciprocal latency as a dependent variable, Kimble (26) obtained a curve of roughly the expected form for one to 24 hours deprivation (the latter being the training level).<sup>7</sup> With responses to extinction as a dependent variable, Cautela (7) obtained clear evidence of positive acceleration as the curve approached the training deprivation, then a virtually linear decline at higher deprivations. Still more pertinent is a study by Horenstein (21) in which both kinds of measures were plotted; the rate curve exhibited negative acceleration and the responses-to-extinction

<sup>7</sup> Both Kimble and Saltzman and Koch (37) report little or no increase in response strength during the first half to one hour after satiation.

curve positive acceleration as the training deprivation was approached.

Zeaman and House (50) have reported an experiment on response strength at "zero" drive as a function of number of reinforcements at high drive which combines in an interesting way the relationships brought out in this section and the one preceding. When acquisition is conducted at a high drive, all drive cues present, and including of course those common to the high drive and a low drive at which tests are to be made, will have increasing probabilities of association with the reference response as the number of reinforcements increases. Consequently if tests are made on a low drive after varying numbers of reinforcements on a high drive, probability of response on the low drive should increase monotonically, and in fact should trace out a curve of virtually the same form as a learning curve obtained under reinforcement at the low drive.

4. *Effects of drive upon gradients of stimulus generalization.* It seems to be generally believed (18, 30) that variations in drive have strong effects upon stimulus generalization, but I have not been able to find much quantitative evidence pertaining to the presumed relationships. A study by Rosenbaum (36) with human subjects indicates that the generalization gradient of a motor response along a height dimension is elevated and flattened by the addition of an irrelevant drive, but the possibility of a converse effect was not investigated. According to Hull's theory, variation in  $D$  should simply multiply any given generalization gradient through by a constant; just how, if at all, this picture should be complicated by the effects of drive stimuli, I have not been able to determine from published sources.

Relationships predicted from the present stimulus theory are illustrated in Fig. 6. The type of stimulus dimension assumed in this example is one such as wave length of light or tonal frequency, over which generalization gradients uncomplicated by changes in drive are probably essentially linear for a considerable range (18). For computational purposes, I have taken response probability to the CS at the training value equal to unity and

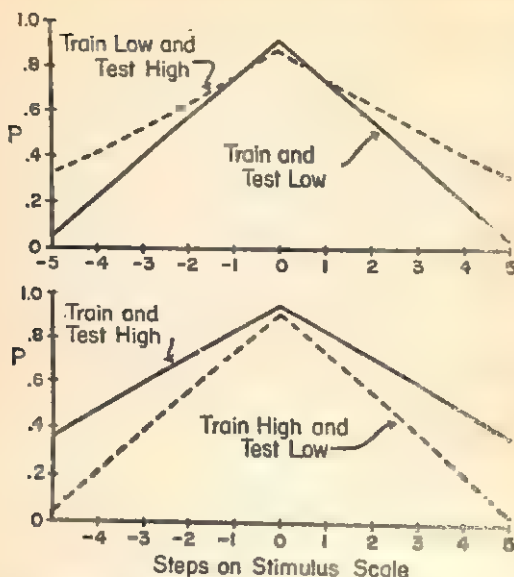


Fig. 6. Effects of deprivation upon stimulus generalization.

have assumed the overlap between pairs of adjacent test stimuli to be such that the gradient, in the absence of drive cues, would fall to zero at plus and minus five steps on the abscissa. Taking into account drive cues, which would be constant over the set of tests, flattens the gradient slightly at a low deprivation and more at a high deprivation, as shown by the solid lines in the figure. If after training at a low drive, the drive is increased (dashed lines in upper panel) the weight of the cues common to all test situations is increased, and the gradient is lowered and further flattened. If after training at a high drive, the drive is lowered before testing (dashed lines in the lower panel of the figure), the weight of common cues is reduced and the gradient is lowered and steepened.

5. The "energizing" effects of irrelevant drives. Before we can deal with experiments involving two or more experimentally defined drives, we require some definite

assumptions concerning the presence or absence of overlap between the sets of cues associated with the various drives. Considering hunger and thirst, we must conclude from the extensive literature on drive discriminations, cited in an earlier section, that the sets of cues controlled by food and water deprivation do not wholly overlap. On the other hand, there is equally convincing evidence that these sets do partially overlap. Take, for example, a series of experiments conducted by Bishop (3) in the Indiana University laboratory. In the first experiment, rats were reinforced for bar pressing (a) 400 times with water while thirsty, (b) 800 times with water while thirsty, or (c) 400 times with water while thirsty and 400 times with food while hungry. Then all groups were given extinction first while thirsty and later while hungry, with the following results in terms of frequency of extinction responding.

<i>Acquisition</i>	Ext. 1 (T)	Ext. 2 (H)
400 H, 400 T	171	50
800 T	144	28
400 T	109	19

Differences between the first and third rows were significant for both periods and between the first and second rows for the second period. The fact that the group with the additional 400 reinforcements on hunger yielded significantly more extinction responding during *both* periods than the group with only 400 reinforcements on thirst supports the assumption that the two deprivational conditions have common cues. A companion experiment with the following design and results (all differences being significant except those between the first and second rows of Ext. 1 and the second and third rows of Ext. 2) provides further support; the 400 additional reinforcements on thirst yield significantly more extinction responding on both drives than 400 on hunger alone.

<i>Acquisition</i>	Ext. 1 (H)	Ext. 2 (T)
400 H, 400 T	101	43
800 H	99	27
400 H	76	20

These findings, together with those of Porter and Miller (32), seem to warrant the assumption that the sets of drive stimuli associated with hunger and thirst have common elements throughout the range of deprivation times.

The conceptual basis for predictions generated by the present theory for the type of "irrelevant drive" experiment reported by Webb (43) and Brandauer (5) is illustrated in Fig. 7. For simplicity I have assumed the

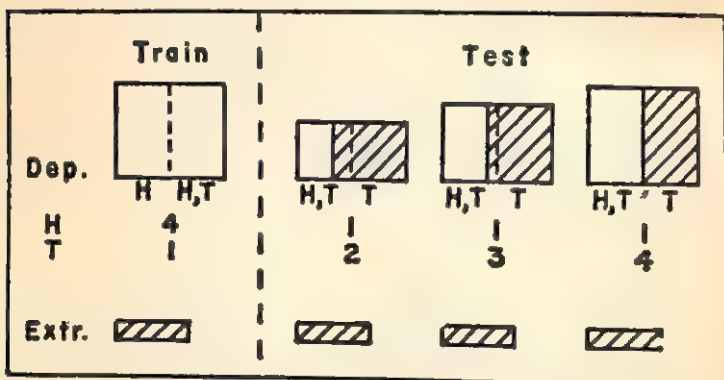


Fig. 7. Schema illustrating theoretical basis for predictions concerning "irrelevant drive" experiments of the type reported by Webb and Brandauer.

proportion of overlap between food-deprivation and water-deprivation cues to be constant at all deprivation times (and have omitted CS cues and Satiation cues, whose inclusion would not affect the predictions materially). When training is given at the highest degree of food deprivation, the reference response becomes conditioned to cues associated with both hunger and thirst. Some of the latter will be present during the test periods, the number and weight both being increasing functions of degree of water deprivation. The remainder of the drive cues present on test periods are associated with water-deprivation alone and thus have had no opportunity to become conditioned. Computing response prob-

abilities in the usual manner, we arrive at the values shown in Table 3. We see that response probability increases with degree of water deprivation, but approaches a limit much below the response probability on the training condition—a pattern which agrees quite well with that manifested in Webb's data.

### SUMMARY

It has become progressively clearer over a period of years that accumulating data on "drive discriminations" and certain types of "irrelevant drive" manipulations virtually force upon us the concept of drive stimuli. In this paper we have made some specific, though provisional, assumptions about the quantitative properties of drive stimuli associated with such deprivational conditions as hunger and thirst. The testable consequences of these assumptions turn out to include detailed predictions of many empirical relations that have customarily been taken as evidence for the *D* construct of Hull and Spence. In the light of our findings, the fact that a

TABLE 3 Irrelevant Drive Experiment

	Theoretical Data				
	Training		Test		
	High	Low	Medium	High	
Food Deprivation	.....	.....	.....	.....	
Water Deprivation	.....	.....	.....	.....	
Response Probability	.94	.36	.43	.47	
	Webb's Data				
	Training		Test		
Food Deprivation	22	0	0	0	0
Water Deprivation	0	0	3	12	22
Response Frequency	14.2	2.8	5.2	5.1	7.2

multiplicative relation between habit strength and degree of deprivation is observed to hold over suitably restricted intervals in various experimental situations cannot be

taken as strong support for the Hullian conception of a general energizing factor.

The model we have investigated in this paper must survive further tests, and may well undergo substantial revision in matters of technical detail, before it can be admitted to good standing as an explanatory theory. For the moment, we may claim a reasonable day's work if we have brought out clearly the existence of hidden riches in the hitherto overshadowed concept of drive stimuli and have put the concept in better form for experimental tests and applications.

### References

1. BAILEY, C. J. The effectiveness of drives as cues. *J. comp. physiol. Psychol.*, 1955, 48, 183-187.
2. BAILEY, C. J. and PORTER, L. W. Relevant cues in drive discrimination in cats. *J. comp. physiol. Psychol.*, 1955, 48, 180-182.
3. BISHOP, C. K. Summation and generalization of response strength in relation to hunger and thirst drives. Ph. D. dissertation, Indiana Univ., 1953.
4. BLOOMBERG, R., and WEBB, W. B. Various degrees within a single drive as cues for spatial response learning in the white rat. *J. exp. Psychol.*, 1949, 39, 628-636.
5. BRANDAUER, C. M. A confirmation of Webb's data concerning the action of irrelevant drives. *J. exp. Psychol.*, 1953, 45, 150-152.
6. CAMPBELL, B. A., and SHEFFIELD, F. D. Relation of random activity to food deprivation. *J. comp. physiol. Psychol.*, 1953, 46, 320-322.
7. CAUTELA, J. R. Experimental extinction and drive during extinction in a discrimination habit. *J. exp. Psychol.*, 1956, 51, 299-302.
8. COTTON, J. W. Running time as a function of amount of food deprivation. *J. exp. Psychol.*, 1953, 46, 188-198.
9. ENINGER, M. U. The role of irrelevant drive stimuli in learning theory. *J. exp. Psychol.*, 1951, 41, 446-449.
10. ESTES, W. K. Toward a statistical theory of learning. *Psychol. Rev.*, 1950, 57, 94-107.
11. ESTES, W. K. Statistical theory of spontaneous recovery and regression. *Psychol. Rev.*, 1955, 62, 145-154.
12. ESTES, W. K., and BURKE, C. J. A theory of stimulus variability in learning. *Psychol. Rev.*, 1953, 60, 276-286.
13. ESTES, W. K., and LAUER, D. W. Conditions of invariance

- and modifiability in simple reversal learning. *J. comp. physiol. Psychol.*, 1957, 50, 199-206.
14. FINAN, J. L. Quantitative studies in motivation. I. Strength of conditioning in rats under varying degrees of hunger. *J. comp. Psychol.*, 1940, 29, 119-134.
  15. FINGER, F. W., Reid, L. S., and Weasner, M. H. The effect of reinforcement upon activity during cyclic food deprivation. *J. comp. physiol. Psychol.*, 1957, 50, 495-498.
  16. GRICE, G. R., and DAVIS, J. D. Effect of irrelevant thirst motivation on a response learned with food reward. *J. exp. Psychol.*, 1957, 53, 347-352.
  17. GUTHRIE, E. R. *The Psychology of Learning* (Rev. Ed.). New York: Harpers, 1952.
  18. GUTTMAN, N., and KALISH, H. I. Experiments in discrimination. *Sci. American*, 1958, 198, 77-82.
  19. HEBB, D. O. Drives and the C. N. S. (conceptual nervous system). *Psychol. Rev.*, 1955, 62, 243-254.
  20. HERON, W. T. Internal stimuli and learning. *J. comp. physiol. Psychol.*, 1949, 42, 486-492.
  21. HORENSTEIN, BETTY R. Performance of conditioned responses as a function of strength of hunger drive. *J. comp. physiol. Psychol.*, 1951, 44, 210-224.
  22. HULL, C. L. Differential habituation to internal stimuli in the albino rat. *J. comp. Psychol.*, 1933, 16, 255-273.
  23. HULL, C. L. *Principles of Behavior*. New York: Appleton-Century-Crofts, 1943.
  24. JENKINS, J. J., and HANRATTY, J. A. Drive intensity discrimination in the albino rat. *J. comp. physiol. Psychol.*, 1949, 42, 228-232.
  25. KENDLER, H. H. Drive interaction. I. Learning as a function of the simultaneous presence of the hunger and thirst drives. *J. exp. Psychol.*, 1945, 35, 96-109.
  26. KIMBLE, G. A. Behavior strength as a function of the intensity of the hunger drive. *J. exp. Psychol.*, 1951, 41, 341-348.
  27. LEEPER, R. The role of motivation in learning: a study of the phenomenon of differential control of the utilization of habits. *J. genet. Psychol.*, 1935, 46, 3-40.
  28. LEVINE, S. The role of irrelevant drive stimuli in learning. *J. exp. Psychol.*, 1953, 45, 410-416.
  29. LEWIS, D. J., and COTTON, J. W. Learning and performance as a function of drive strength during acquisition and extinction. *J. comp. physiol. Psychol.*, 1957, 50, 189-194.
  30. MILLER, N. E. Theory and experiment relating psycho-

- analytic displacement to stimulus-response generalization. *J. abnorm. soc. Psychol.*, 1948, 43, 155-178.
31. PERIN, C. T. Behavior potentiality as a joint function of the amount of training and the degree of hunger at the time of extinction. *J. exp. Psychol.*, 1942, 30, 93-113.
  32. PORTER, L. W., and MILLER, N. E. Training under two drives, alternately present, vs. training under a single drive. *J. exp. Psychol.*, 1957, 54, 1-7.
  33. RAMOND, C. K. Performance in selective learning as a function of hunger. *J. exp. Psychol.*, 1954, 48, 265-270.
  34. REID, L. S., and FINGER, F. W. The effect of activity restriction upon adjustment to cyclic food deprivation. *J. comp. physiol. Psychol.*, 1957, 50, 491-494.
  35. REYNOLDS, B. The relation between the strength of a habit and the degree of drive present during acquisition. *J. exp. Psychol.*, 1949, 39, 296-305.
  36. ROSENBAUM, G. Stimulus generalization as a function of level of experimentally induced anxiety. *J. exp. Psychol.*, 1953, 45, 35-43.
  37. SALZMAN, I., and KOCH, S. The effect of low intensities of hunger on the behavior mediated by a habit of maximum strength. *J. exp. Psychol.*, 1948, 38, 347-370.
  38. SCHOEFFLER, M. S. Probability of response to compounds of discriminated stimuli. *J. exp. Psychol.*, 1954, 48, 323-329.
  39. SPENCE, K. W. *Behavior theory and conditioning*. New Haven: Yale Univ. Press, 1956.
  40. STRASSBURGER, R. C. Resistance to extinction of a conditioned operant as related to drive level of reinforcement. *J. exp. Psychol.*, 1950, 40, 473-487.
  41. TEEL, K. S. Habit strength as a function of motivation during learning. *J. comp. physiol. Psychol.*, 1952, 45, 188-191.
  42. VERPLANCK, W. S., and HAYES, J. R. Eating and drinking as a function of maintenance schedule. *J. comp. physiol. Psychol.*, 1953, 46, 327-333.
  43. WEBB, W. B. The motivational aspect of an irrelevant drive in the behavior of the white rat. *J. exp. Psychol.*, 1949, 39, 1-14.
  44. WICKENS, D. D., HALL, J., and REID, L. S. Associative and retroactive inhibition as a function of the drive stimulus. *J. comp. physiol. Psychol.*, 1949, 42, 398-403.
  45. WILLIAMS, S. B. Resistance to extinction as a function of the number of reinforcements. *J. exp. Psychol.*, 1938, 23, 506-522.

46. WOODWORTH, R. S. *Dynamic Psychology*. New York: Columbia Univ. Press, 1918.
47. YAMAGUCHI, H. G. Drive ( $D$ ) as a function of hunger ( $h$ ). *J. exp. Psychol.*, 1951, 42, 108-117.
48. YAMAGUCHI, H. G. Gradients of drive stimulus ( $S_D$ ) intensity generalization. *J. exp. Psychol.*, 1952, 43, 298-304.
49. YOUNG, P. T. *Motivation of Behavior*. New York: Wiley, 1936.
50. ZEAMAN, D., and HOUSE, BETTY J. Response latency at zero drive after varying numbers of reinforcements. *J. exp. Psychol.*, 1950, 40, 570-583.

## Comments on Selected Controversial Issues

NEAL MILLER

*This article seems to us to be a fitting conclusion to this book because it tries to give an overview of ideas of motivation used by learning theorists and, at the same time, shows how various findings can be fitted into one theory. It also shows the feeling that the end is not yet. That present theories probably do not contain the ultimate theory of motivation and that the search must, and will, go on.*

This part of the paper continues the general discussion of the way in which I differ from certain other stimulus-response psychologists by a brief discussion of a number of controversial issues.

### DO DRIVES FALL INTO THE CLASS OF STIMULI?

The notion that characteristic stimuli are associated with drive states is relatively old but not completely accepted. I have explored some of the consequences of carrying this notion to its logical conclusion by assuming that all drives are strong stimuli and that any stimulus becomes a drive if intense enough [76].

One deduction from this assumption is that drive generalization should occur as a special case of stimulus generalization. This deduction has been tested in a series of experiments, first briefly summarized [57] and then reported in more detail after World War II [61]. The first of these experiments showed that, if thirsty rats are

---

Reprinted from *Psychology: A Study of a Science*, Vol. 2; edited by Sigmund Koch. New York: McGraw-Hill Book Co., Inc., 1959, with permission of the author and publishers.

trained to run down a short alley for water and then tested without thirst, they will not only run faster but also drink more if tested with the irrelevant drive of hunger than if tested without this irrelevant drive. Since hunger and thirst are so closely related, a second experiment was performed in which hungry rats were trained to take the correct arm of a T maze to secure food. Subsequent tests without hunger showed that they would both run faster and choose more correctly if tested with the irrelevant drives of either electric shock or fear than if tested without these additional drives.

Additional evidence was subsequently secured in our laboratory by von Felsinger [107] who showed that induced estrus (presumably involving sexual motivation) increased the running speed and improved the error scores of two groups of satiated rats which had originally been trained to run a maze when hungry or thirsty respectively. Similarly, Webb [108] at Iowa found that thirst as an irrelevant drive increased the performance by food-satiated animals of a habit originally learned under hunger. Although Webb's experiment had the advantage of using more than two levels of irrelevant drive, his thirsty animals may have stopped eating dry food and thus been somewhat hungry when tested.

Hull [39], who was familiar with the results of the first experiments in this series, interpreted them in a different way. Instead of a gradient of drive-stimulus generalization, he assumed a completely general energizing effect, possibly mediated by substances in the blood. Aside from the fact that the blood-substance subhypothesis would be too slow to account for rapid response to pain, the chief difference between Hull's hypothesis and mine is that I would predict the type of relationships that would be expected from a gradient of stimulus generalization. For example, if irrelevant drive A had more effect on a habit based on drive B than drive C, then a habit based on drive A should be more effected by B as an irrelevant drive than by C. By contrast, Hull's 1943 postulate predicts completely equal effects, although his 1952 one is more guarded in this respect. The experiments required to decide between these two main alternatives have not yet been performed.

*Response-produced Drives and Rewards*

By combining the hypothesis that strong stimuli serve as drives with the notion of response-produced stimulation, one arrives at the hypothesis that learnable drives are the product of responses that produce intense stimulation. One also arrives at the hypothesis that secondary reinforcement is produced by responses that reduce strong stimulation. It should be remembered that these responses might occur within the central nervous system. These hypotheses, which have led to a considerable amount of significant work, have been presented in detail elsewhere [64, 25]. Some further thoughts will be presented in a subsequent section on learned elicitation vs. learned channeling.

*Drive Stimulation vs. Reduction in Threshold*

A number of workers [96, 4, 99] have assumed that, instead of acting as strong stimuli, drives serve to reduce thresholds. One of the original points of departure was Richter's [96] observation that adrenalectomized rats would prefer saline to tap water, even with low saline concentrations that elicited no differential response from normal control rats. This better discrimination of the rats with great salt need was interpreted as a reduction in the threshold for the taste of salt, produced by the specific need or drive. Subsequent work has demonstrated that the threshold of the sense organs, measured electrophysiologically, remains the same [94], and that, if normal rats are properly motivated by electric shock, they can discriminate just as well as adrenalectomized ones [34]. Thus the original support is pulled out from under the threshold hypothesis.

Meanwhile, however, Beach and Holz-Tucker [4] were led to the threshold hypothesis by the observation that increasing the androgen level in the blood of male rats increases the range of stimulus objects to which they will exhibit copulatory responses. As we have already pointed out in the earlier part of this paper, such an observation could just as well be explained by assuming that increases in drive raise the level of the entire gradient of stimulus generalization.

It should be clear that going through the experimental operations to demonstrate a reduction in threshold of response to a stimulus does not rule out the possibility that this reduction was produced by the presence of some other source of stimulation. The traditional physiological and psychological experiments on the positive summation of stimuli demonstrate that the threshold of response to the test stimulus is reduced by the presence of the one with which it summates. Conversely, if one deals with the central rather than the peripheral effects of stimulation, it is obvious that a genuine reduction in threshold might cause a stimulus to function as though it were intensified. It is actually quite difficult to discriminate between these two hypotheses: drive as a strong stimulus, or drive as a state which reduces the threshold of peripheral end organs or central synapses.

Sheffield and Campbell [99] and Campbell and Sheffield [18] have presented evidence purporting to support the threshold hypothesis by disproving the drive-stimulus one. They show that, if a rat is kept under relatively soundproof conditions, his rate of activity does not go up when he is hungry, but that the introduction of a novel stimulus will greatly increase the activity of the hungry rat and still more increase it if the novel stimulus is regularly associated with feeding. It is assumed that, if drive were a stimulus, it would have to produce similar increases in activity. Since such an increase was not observed, it is concluded that the function of the drive must have been solely to decrease the threshold of responsiveness to the novel stimulus, an assumption functionally similar to Hull's [39] postulate that drive serves to multiply whatever habit strength is present.

Although these experiments show that drives do not inevitably goad the animal into activity, they do not rule out the possibility that drives are strong stimuli. An electric shock is a strong stimulus. Whether an animal shows increased activity by jumping or decreased activity by crouching to an electric shock depends on whether the reward of turning off the shock is administered for jumping or for crouching. It will be remembered that my definition of drive says nothing about activity, but deals rather with increasing the performance

of responses *rewarded* by the offset of the drive or by the goal objects that produce its satiation.

In Fig. 1 of the article by Sheffield and Campbell [99], it will be noticed that, whereas the activity in response to a stimulus that immediately precedes daily feeding, increases during a series of trials, the activity to a control stimulus that does not precede feeding progressively decreases. It seems logical to me to assume that the non-rewarded responses to the latter stimulus gradually extinguish and that the nonrewarded responses to the gradually mounting drive stimulus also extinguished in the context of the other constant cues in the Sheffield-Campbell apparatus.

To state the converse side of the argument, suppose that at unpredictable intervals, without providing any stimulation, the experimenters had introduced food alternately at opposite sides of their activity apparatus throughout the test session. Then the animals would have been reinforced for shuttling on a variable-interval schedule. We would expect their shuttling, which would be recorded as activity, to increase with increasing hunger just as does the rate of bar pressing [67]. In short, we can only predict that subjects will be active to a pattern of stimulation including strong drive when activity has been reinforced to that pattern. The situation used by Sheffield and Campbell [99] involved long periods during which activity was not reinforced to the pattern of drive stimulus plus apparatus without the sound of the food delivery mechanism.

## THE NATURE OF REINFORCEMENT

It seems to be a fact that some stimulus situations are much more effective than others in determining whether or not the responses that lead to them will be learned and performed. Thus, all theorists are forced to assign the empirical law of effect some role in their theories. A consistent application of the empirical law of effect would result in a long list of stimulus situations that can serve as rewards and of those that are relatively neutral. In practice, most experimenters limit themselves

to relatively few situations which are known to function as effective rewards.

### *The Drive-Stimulus-Reduction Hypothesis*

Since a catalogue of rewards is so cumbersome, some theorists have looked for a simple general principle, or principles, that will allow them to determine whether an event should be listed as a reward or not. One attempt at such a principle is the drive-reduction hypothesis of reinforcement.

It is well known that turning off a strong motivational stimulus, such as an electric shock, will serve as a reward to reinforce whatever response the animal was making just before he escaped from the shock. It is also known that food will serve as a reward for a hungry animal, and that this same food, if given in sufficient quantities, will reduce the strength of the hunger drive. The drive-reduction hypothesis attempts to abstract a common element from observations of this kind. In its weak form, it states that the sudden reduction in the strength of any strong motivational stimulus always serves as a reward, or in other words, is a *sufficient* condition for reinforcement. In its strong form, it states that all reward is produced in this way, or in other words, that drive reduction is not only a sufficient but also the *necessary* condition for reinforcement.

By defining drives as strong stimuli, I have sharpened the hypothesis into the assumption that it is the sudden reduction in the strength of intense stimulation that serves as a reinforcement.

### *Not Completely Committed to Drive-Stimulus-Reduction Hypothesis*

Although I believe that the foregoing hypothesis has a considerably less than 50 per cent chance of being correct, especially in its strong form, I do believe it is better at the present moment than any other single hypothesis. Therefore, I feel that it is worthwhile to try out applying it consistently, if only to highlight the obstacles and infuriate others into devising superior hy-

potheses and the experimental programs to support them.

When one systematically explores a given hypothesis and points out the weaknesses of various theoretical and experimental attacks on his hypothesis, however, it is difficult to avoid the reputation (and after that, the fact) of being emotionally fixated on the hypothesis. Furthermore, the very controversial nature of the drive-stimulus-reduction hypothesis makes it conspicuous, so that it seems to be the cornerstone of one's whole theoretical thinking.

Let met try to destroy both illusions. The stimulus-reduction hypothesis of reinforcement could be discarded without having an appreciable effect on the rest of my theoretical formulations. I take this occasion to urge attempts to formulate and rigorously test competing hypotheses, and time permitting, may even join in that activity myself. However unsatisfactory, the drive-reduction hypothesis is not likely to be abandoned as long as it is the best thing of its kind that we have. The decisive way to kill it is with a superior alternative.

#### *Various Possible Bases For Correlation Between Reinforcement and Satiation*

Let us start out with the original observation that there seems to be a correlation between the operations of satiation and those of reinforcement. This is not surprising because animals that are not rewarded by substances that reduce their drives are likely to come from a long line of extinct ancestors. Furthermore, the process could also work the other way—individuals that do not eventually become satiated on a given form of pleasure may starve to death while enjoying the thrill of tickling themselves.

We should remember that a correlation does not necessarily indicate a direct causal relationship. The drive reduction could produce the reinforcement, the reinforcement could produce the drive reduction, or natural selection could have built a correlation into the organism that did not involve any direct causal relationship. For example, an increase in pleasure could be the true basis for reinforcement and animals could have evolved

by natural selection so that in general only nutritious substances taste good and the pleasantness of these is some function of the strength of hunger.

### *Sample Studies Illustrating Fruitfulness of Hypothesis*

If natural selection has produced a spurious correlation, we would expect this correlation to be more likely to disappear if we manipulated drives in unusual ways which have not been encountered in natural selection. On the other hand, if it is a true causal relationship, it should survive these unusual manipulations.

In order to test the drive-reduction hypothesis in this way, and also to learn more about the mechanism of drives, I have been involved in a series of experiments which have tried to induce and reduce drives in unusual ways [73, 7, 78]. These experiments have shown that food injected via a plastic fistula directly into the stomach of a rat promptly reduces hunger as measured either by the amount of food subsequently eaten or by the rate at which the animals will work at pressing a bar to secure food on a variable-interval schedule of reinforcement. But food taken normally by mouth has a greater effect on both of these two measures. Similar results have been secured for thirst, which has also been induced by injecting hypertonic saline into the ventricles of the brain and reduced by injecting water. In short, there seem to be a number of mechanisms—located in the mouth, stomach, and brain—for regulating drive.

Furthermore, exactly as the drive-reduction hypothesis would predict from the foregoing facts, food injected directly into the stomach will serve as a reward to cause rats to choose the correct side of a T maze, but food taken normally by mouth will serve as a stronger reward to produce more rapid learning. The rewarding effect of food injected directly into the stomach would not be expected from hypotheses that used either the pleasant taste or the consummatory response as the sole basis of reinforcement. Conversely, the saccharine solution which Sheffield and Roby [100] proved was a reward for hungry animals has been found by us to reduce hunger [67].

*Effects of electrical stimulation of the brain.* Another series of studies has demonstrated the possibilities of motivating learning and performance by direct electrical stimulation of the brain [83, 23, 70]. In connection with these studies, I have discussed [71] the implications for theories of reinforcement of the reward effect discovered by Olds and Milner [92] and have shown how the drive-stimulus-reduction hypothesis can suggest significant new studies of central motivation and reward—studies which ultimately may lead to a superior hypothesis. A summary of my recent physiological studies of motivation has been published elsewhere [73].

*Campbell's psychophysical studies.* The hypothesis that drives are strong stimuli and that sudden reductions in them produce reinforcement, has further demonstrated its usefulness by starting Byron Campbell on a series of studies which extend the psychophysical tradition to a new area by quantitatively studying the rewarding effects of sudden reductions in the strength of strong stimuli. Electric shocks and loud sounds have been used in these studies which show that, when the stimulation is reduced to zero, stronger initial strengths of stimulation produce greater learning and performance. But when the motivation is not reduced to zero, a given absolute amount of reduction is less effective the higher the initial level. Within the middle ranges, equal reinforcing effects seem to be produced by equal ratios of reduction, a relationship like Weber's law, which breaks down at the extremes. Thus, what might be described as a "reinforcement threshold" seems to show the same general type of curve as does the psychophysical threshold for a just noticeable difference, although much larger changes are required to produce a reinforcing effect [17, 15, 16].

### *Relationship of Drive to Learning*

A stimulus-response analysis can clarify some of the apparent confusion about the relationship of drive to learning [66]. In the practical teaching situation, everyone knows that poorly motivated students do not learn. This is chiefly because they do not expose themselves to the proper cues or perform the correct responses, for example, open their books and study. But most experi-

ments on this topic have concerned themselves with a somewhat different problem: the relationship of drive to reinforcement. Therefore, they have tried to control the factors of exposure to cues and number of responses by giving both groups the same number of trials in very simple situations. One must be careful in generalizing from this kind of experiment to the practical situation; a film of mine [77] yields a better analogy.

When the drive is promptly reduced to zero after the correct response, there is a perfect correlation between the strength of drive and the amount of drive reduction. Thus, according to the stimulus-reduction hypothesis, stronger drives should produce more reinforcement and better learning. These conditions are met in most experiments on escape learning and eyelid conditioning. The results of such experiments agree with the theory.<sup>1</sup>

In the work of Campbell and his collaborators, we have just seen that when the drive stimulus is reduced a *constant absolute value*, the amount of reinforcement follows the same Weber's law principle as other stimuli; it is less at the higher levels of drive. Under these circumstances, the intense-stimulus-reduction hypothesis would not expect a positive relationship between the strength of drive and amount of learning.

In experiments in which the correct response is reinforced by pellets of food or sips of water, it is obvious that, at the extreme low end of the continuum, there will be little possibility of drive reduction if there is almost no drive present. And indeed, experiments on so-called latent learning seem to agree that extremely low drives (whether all relevant primary and secondary drives are zero is controversial) seem to produce less learning than does higher drive.

When we start comparing moderate drives with strong ones, however, we simply do not know what the relationship is between the strength of drive and the *relative* amount of drive reduction produced by a single pellet of food or sip of water. Therefore, it should not be too

<sup>1</sup> It should be noted, however, that all such experiments have involved noxious stimuli so that one might claim that the positive results were produced by a learned drive, such as fear, which persisted from training to testing.

surprising that the results of such experiments are confusing. Even if the animal were fed to satiation at the end of a trial, the total amount of drive reduction would be confounded with considerable delay. Perhaps techniques for manipulating hunger and thirst by electrical stimulation of the brain (or measuring them by central recording) will develop to the point where we can conduct decisive experiments on such drives.

Spence [103] has clearly pointed out and documented an additional source of possible confusion, namely, that whenever an incorrect response is dominant at the beginning of learning, Hull's [39] principle that drive multiplies the strength of habits predicts that increases in drive will increase its excitatory potential more than that of the subdominant correct responses. To this and other excellent points that he makes, I would add only that one must consider (1) possible innate responses to different strengths of drive, and (2) the possibility that rates and amplitudes are specific responses as would be expected from a micromolar analysis of the type expounded by Logan [49].

*Large Areas in Which Lack of Suitable Measures Makes Hypothesis Inapplicable*

In experiments using extremely strong stimuli, such as electric shocks, intense lights, or sounds that are under external control, the stimulus-reduction hypothesis clearly applies and is routinely verified. But perhaps the most damning thing that can be said about this hypothesis is that there are so many situations in which, with our present techniques, we cannot tell for certain whether the net effect of a given experimental operation is to produce an increase or a decrease in the motivating stimulus. Although such limitations are not unique to this hypothesis, a clear-cut possibility of disproof would be preferable to the present ambiguity. For example, if the change from complete darkness to weak illumination is reinforcing, it can be argued that the small increase in visual stimulation is more than overbalanced by a reduction in boredom or fear. Similarly, it can be argued either that tiny pellets of food produce brief reductions in drive, or that they do not appreciably reduce hunger

and may even whet the animal's appetite. Guthrie [31] can argue that a boy banging on a drum is seeking stimulation and Harlow [32] can say the same for the curiosity of his monkeys. On the other hand, a plausible case can be made that both boy and monkey are escaping from the intolerable tension of boredom. Such ambiguities have motivated me to the program of research aimed at trying to learn more about the mechanisms of drive induction and reduction. Perhaps there are physiological ways of securing independent measures of at least certain drives, or behavioral techniques for trying to extend the correlation between satiation and reinforcement to other drives such as curiosity [73, 91].

### *Alternative Hypotheses*

Guthrie's [31] version of the pure contiguity hypothesis is enormously appealing because of its simplicity and because it would be relatively easy to imagine a physiological mechanism that worked in this way. I have been unable to see, however, why it should not predict that a thoroughly learned response to a momentary stimulus will never extinguish. After a given response is learned to the point where it regularly is the first and last response to that stimulus, should not more and more of the atypical members of the population of cues be conditioned to the response so that it would become progressively more certain to occur?

There is also the problem of why the stimulus-change produced by the onset of a painful stimulus does not protect preceding responses from retroactive inhibition. One might modify Guthrie's hypothesis to avoid these difficulties by assuming that only the termination of a stimulus (and not changes produced by adding new cues) functions to protect responses from retroactive inhibition, and that such protection is proportional to the number of cue elements that are removed. Then, the effect of terminating a weak stimulus would be negligible because the change in the total population of cue elements would be so small, but the termination of a strong stimulus would be more effective because many more cue elements would be removed. With such modifications, Guthrie's application of the contiguity hypothesis

would become functionally very similar to the hypothesis that reinforcement is produced by the prompt reduction in a strong stimulus.

*Various two-factor theories have considerable attractiveness.* It is easy to imagine a simple contiguity mechanism being evolved at an early stage and then supplemented with some sort of booster effect from reward or the escape from punishment. Indeed, it is quite possible that careful research on primitive organisms, or on more complex ones primitivized by removing higher segments of the brain, would show that it is possible for them to learn several responses by simple conditioning but not to select among them by trial and error, even when the proper conditions for trial and error learning are realized.

Actually, the shift from a single factor to a multifactor theory need not be very disruptive to the general features of such miniature systems as I have proposed as long as most of the laws—gradient of reinforcement, stimulus generalization, experimental extinction, spontaneous recovery, etc.—remain the same [25, p. 42]. These laws seem to be the same for classically conditioned autonomic responses as well as for trial-and-error somatic responses reinforced by either escape from electric shock or rewarding a hungry animal with food.

To use a simple analogy, to change the lighting in an apartment house from electricity to gas, would be a major undertaking because the two sources of energy follow completely different laws, and thus would require changes such as those from wires to pipes. A change from a-c to d-c current would involve relatively minor alterations, since these two types of current have relatively similar functional properties. A shift from alternating current delivered by a private company to alternating current delivered by a municipal one would involve only a small change in the connections to the basement (which, indeed, would be highly significant to those who sell the current from different sources), but there would be no changes in the wiring and appliances throughout the apartment house since the current from these two sources follows identical laws.

Proponents of multiple-factor theories of learning can attract more serious attention if they can rigorously spe-

cify different laws associated with supposedly different types of learning.

*Cognitive theories.* S-R theorists are confronted with the problem of explaining man's obviously intelligent behavior; cognitive theorists are confronted with the problem of explaining obvious stupidity. Although recognizing that much of man's behavior involves cognitions, I have preferred the strategy of trying to explain such behavior as the outgrowth of simpler, noncognitive mechanisms. It is hard for me to conceive of cognitive insight as the sole means of acquiring maladaptive neurotic symptoms or those many motor skills that seem to be almost entirely unconscious.

One obvious alternative taken by many cognitive theorists is to assume different levels of learning—a lower S-R one to explain stupid behavior and a higher cognitive one to explain intelligent behavior. This may turn out to be the most parsimonious explanation possible, but the theorist will have to be careful to specify under what conditions both occur or else he will be limited to *post hoc* explanations. Another alternative is to adopt the general pattern of Tolman's theory—using association as the basic element of learning, and the empirical law of effect as the mechanism of activation—but stating the laws in terms of automatic processes. Spence's [103] theory is tending in this direction. Variations of this general pattern certainly should be rigorously explored. But one must be careful that the theory does not predict that, if rats or earthworms show any learning with motivation and reward, they should show approximately as good latent learning.

#### NEW TRAILS FOR RESEARCH ON MOTIVATION<sup>2</sup>

We are still a long way from a satisfactory understanding of motivation. To take a simple example, much of our human social behavior is overdetermined by many different sources of drive and reinforcement. Thus, money is the focus of many needs; its possession the means to

<sup>2</sup> Throughout this section I frequently use the term motivation to include the effects of both drive and reward, using the latter terms if I want to distinguish the two effects.

many rewards. But our experimental literature has scarcely begun [95] to tackle the problems of how different sources of drive and reward summate positively or negatively in the learning and maintenance of a given habit.

### *Learned Elicitation vs. Learned Channeling*

Most of the motivation of adult human social behavior is either acquired by or profoundly modified by learning. I have called such motivations *learnable drives and rewards* to emphasize the fact that the basic physiological mechanism is probably innate, although the arousal or reduction in response to specific cues is a product of learning [64]. In the case of fear, it seems possible for part of the response innately elicited by pain (loud sounds and perhaps a considerable variety of other situations) to be conditioned to new cues so that it can be elicited in the complete absence of any obvious, relevant primary drive. Let us call this *learned elicitation*.

I have assumed that the elicitation of a number of social drives might be learned in the same way as fear, for example, as a result of thousands of instances in which feeding and other sources of infant gratification are associated with the appearance of an approving parent. However, I have repeatedly failed to establish experimentally any appreciable learned elicitation of drive on the basis of primary drives such as hunger or thirst [cf. 91].<sup>3</sup>

Although this failure has not caused me to abandon completely the attempt to apply the fear-elicitation paradigm to appetitive drives, it has caused me to shift the emphasis in a new direction. As pointed out earlier

<sup>3</sup> It is conceivable that this failure is due to the fact that these drives are aroused so slowly that one is always dealing with delayed, or even backward, conditioning. That is one of the reasons why I have been interested in exploring possibilities of eliciting drives rapidly by techniques such as electrical or chemical stimulation of the brain [73]. On the other hand, it may be that such failures are due to the absence of any mechanism for a response-produced drive like the one involved in fear.

[64], even the primary drives themselves may be profoundly modified by learning, so that hunger becomes the desire for a particular kind of food prepared according to culturally determined standards of what is appetizing. Thus, one must learn to like snails or oysters. But the elicitation of the appetite for such foods never becomes completely independent of hunger, since even these delicacies have little appeal after complete satiation by a Thanksgiving dinner. Furthermore, although the taste for snails and other exotic foods is learned, it is continually reinforced by eating them when hungry. Therefore, one does not need to worry about the problem of experimental extinction vs. functional autonomy.

It should be noted, however, that hunger probably can be channeled only to certain categories of objects, namely, foods. I venture that it would be much more difficult to develop and maintain an appetite for sawdust. Are there other drives that permit more latitude in channeling? What are the defining characteristics of their potential goal objects?

The desire for a specific kind of food may be called *learned channeling of drive*. It may be contrasted with the fear of a previously neutral cue which we have just described as learned elicitation of drive.

The foregoing two examples—channeling of hunger and elicitation of fear—may not be completely different, but represent the extremes of a continuum. An intermediate case could occur if there is a latent fear of some type of situation which can be further strengthened by learning. Or there might be an innate tendency to fear some general class of situations which can be made more specific by learning. Furthermore, we might find that the reaction to a conditioned fear stimulus would vary with the general level of anxiety of the subject and be influenced by physiological factors such as endocrine changes.

### *Can Rewards for One Drive Channel a Different One?*

Another possibility is that reinforcement by the goal objects of a given drive may help to determine the direc-

tion in which a second one is channeled.<sup>4</sup> If such channeling occurs, it might easily be mistaken for the establishment of learned elicitation, and/or secondary reinforcement that became independent of the first drive.

A possible illustration of such a channeled drive is the Freudian hypothesis that sex begins as a dependent drive (*Anlehnungstriebe*), so that a young man's eventual object choice may be determined by earlier rewards in the nursing situation under the primary drive of hunger. If this were the case, it is clear that the young man's attraction to his bride might continue to be reinforced by adult sexual (and other) gratifications even though he discovered that she could not cook.

To take a simpler example, Scott [98] observed that two bottle-fed sheep showed no tendency whatsoever to follow the rest of the herd whereas several thousand normally nursed controls all showed strong gregarious tendencies. This observation suggests that gregariousness in sheep may be influenced by learning during the nursing situation. But some special innate susceptibility or additional source of motivation must be involved since not all mammals are as gregarious as sheep.

Similarly, a variety of obvious rewards, such as feeding, determine a dog's attachment to a particular master or family. This attachment may be strong enough to motivate the dog to learn to respond to various cues by being ready to leap into the car so that he will not be left behind. But equally well-treated cats usually do not acquire a similar motivation. Such observations raise the possibility that, as part of the pattern of hunting in packs, evolution has produced in dogs some special motivational mechanism that can be channeled either to the pack or to the adoptive human family.

The "imprinting" of ducks to follow people and other objects may be an extreme example of channeling. We have already suggested that this "motivation to follow" probably could be used as a drive for the trial-and-error

<sup>4</sup> I use the more general phrase "reinforcement by goal objects" to emphasize the fact that this hypothesis is not necessarily tied to the drive-stimulus-reduction hypothesis of reinforcement.

learning of whatever response produced the release permitting the baby duck to follow its leader [64]. But the original behavior of the newly hatched duck in the absence of any suitable moving objects to follow suggests some strong original motivation, perhaps fear. Could this motivation also be studied? Would its reduction by a suitable object prove to be the source of reinforcement for the original imprinting? What role is played by feeding and other reinforcements which often are confounded with "imprinting"?

It is possible that the type of behavior which we have described as channeling is nothing more than an array of specific habits. Certainly the line between (or blending of) habit and motivation needs clarification as Brown [13] has ably argued.

What I would like to point out is that such habits seem to have many of the functional properties of drives. Oysters for people, or novel foods for rats, do not function as effective rewards until the subject has learned to eat them. How much of this is a function of mere delay in seizing and swallowing? I venture that at least some of the effect may be due to the elimination of aversions by counterconditioning and the acquisition of secondary reinforcement.

Furthermore, the aroma, sight, or even description of a particular delicacy seems to produce a considerable increase in the motivation of a moderately hungry person. I venture that, under appropriate circumstances, it would also facilitate the trial-and-error learning and performance of any response that circumvented a block to the direct approach to the incentive. Furthermore, Tinklepaugh's [104] observations on monkeys and my own incidental observations of people suggest that, once a learned strong anticipatory goal response for a particular food is aroused, other foods may be less effective as rewards. Again the factor of delay in consumption needs to be controlled.

Finally, we need to know the laws governing the ways in which motivation can be channeled and/or elicited by new cues. For example, what conditions can inhibit the development of intellectual curiosity in children and

what conditions can enhance it? For that matter, what conditions can arrest, strengthen, or channel the curiosity of a cat?

Throughout the preceding discussion we have raised a number of theoretical possibilities. Although the animal examples cited should readily lend themselves to precise experimental analysis, such work has not been done, so that our discussion has had to be highly speculative; many different theoretical possibilities are open.

### *What Drives May Be Elicited or Channeled by Learning?*

Since the conditions of social and individual learning are complex and variable, we will expect learnable drives to exist in a baffling variety of combinations in response to a baffling variety of cues. Thus, any standard list of socially learned motives will only be as simple and as stable as the conditions of learning involved. This may explain the baffling variety of such lists.

One might go on to ask, however, what kind of different physiological mechanisms may underlie learnable drives? It seems plausible that fear might be the underlying mechanism in a number of instances that seem different because different conditions of training have caused different categories of cues to arouse the fear, and other specific categories of cues to serve as the goals because they, respectively, reduce these specific fears. Thus, it is interesting to note that we speak of the fear of failure, the fear of disapproval, the fear of losing money, status, or love.

Similarly, Freudian theory presupposes that sex underlies a large number of superficially different motives.

Do certain social motives, such as curiosity, gregariousness, and jealousy, all have a common mechanism, or does each have distinctive ones? Can such motives be elicited, channeled, or both? If they are channeled, what are the characteristics defining potential goals? We have scarcely made a beginning toward answering some of these questions. If we knew more about some of these other sources of motivation, we might not be so strongly tempted to place such a heavy theoretical burden on

fear. We must solve such problems before we can begin to understand the wonderfully complex web of human social motivation.

*Need to Study New Types of Drive;  
One Way to Do It*

Most of the basic research on motivation to date has been confined to an extremely limited number of so-called primary drives such as hunger, thirst, sex, and pain. There is a great need to study new types of motivation in the laboratory. Our present list of experimentally studied motives is far too short. A promising break away from our old limitations has been made with studies of fear and more recent studies of "curiosity" and "activity." We need to extrapolate this new trend.

*A new view of fear.* The old view of fear as an emotion was inferior in that it stressed innate and disrupting effects of fear and did not even give a clear picture of how these were achieved. The newer and much more powerful understanding is (a) that fear is a drive, like hunger or thirst, which may motivate either adaptive or maladaptive behavior, and (b) that a sudden reduction in the strength of fear serves as a reward to reinforce immediately preceding responses.

The point of departure for this new understanding of fear was Mowrer's [86] trenchant stimulus-response analysis of Freud's [26] insightful paper on the problem of anxiety. Mowrer's pioneering work led me to set up an experimental demonstration that fear has a crucial functional property of drive in that it can motivate the trial-and-error learning and performance of whatever response is followed by a sudden reduction in the strength of fear [58, 60]. I believe that this same experimental paradigm that I applied to fear can be used to study many other sources of motivation.

In my experiment on fear, albino rats were placed in a simple apparatus consisting of two compartments separated by a door. One was white with a grid as a floor; the other was black without a grid. Before training, the animals showed no marked preference for either compartment. Then, they were given a number of trials

during which they received electric shock in the white compartment and escaped into the black compartment through the open door. After this training, the animals would run out of the white compartment, even if no shock was on the grid.

Then, during additional trials without further shocks came the critical test to demonstrate that the rats' running was not the mere persistence of a habit, but also had the crucial property of a drive, namely that it could motivate, and its reduction reinforce, the learning of a new habit. The door, previously always open, was closed. The only way that it could be opened was by rotating, by a fraction of a turn, a little wheel which was above the door. Under these conditions, the rats exhibited trial-and-error behavior and gradually learned to escape from the white compartment by promptly rotating the wheel.

When the conditions were changed, so that only pressing a bar would open the door, wheel turning extinguished and a second, new habit (bar pressing) was learned. Thus, the fear, presumably established by the shocks in the white compartment, was shown to have the same functional properties as a drive such as pain in that it could motivate the trial-and-error performance of response and its reduction could reinforce the immediately preceding response.

This new view that fear is a drive and that fear-reduction is a reinforcement has many consequences. These have been developed in the course of studies in which I have been involved—investigations of behavior in combat [24, 110, 65], and an analysis of neurosis and psychotherapy [25]. In going beyond the experimental studies of animals in vital fear situations, I have chosen to look for "naturally" occurring situations where people were confronted with vital dangers, rather than bringing subjects into the relatively safe laboratory and in effect merely saying "boo!" to them.<sup>5</sup> I shall have space to hint at only a few of the main points gleaned from

<sup>5</sup>My work in the Air Force during World War II convinced me of the difficulties of devising situations that would really frighten eager young aviation cadets without terrifying generals and congressmen.

such studies. Many of the ideas emerged from collaboration with John Dollard.

As soon as fear is thought of as a drive, one notices that it can motivate desirable behavior, such as being alert and resourceful, driving slowly, looking for the source of danger and planning ways to minimize it—for example, buying insurance. Of course, fear can also motivate undesirable behavior such as cowardly running and hiding, cheating, and lying. The most important thing, then, is *not how afraid a man is but what fear motivates him to do.*

Since a sudden reduction in the intensity of fear serves as a reward to strengthen responses, a frightened person will learn those responses he is making when his fear is reduced. If he eventually escapes from the fear by brave, adaptive behavior, he should learn to become more courageous and resourceful; if he temporarily reduces fear by cowardly and maladaptive behavior, he should learn to become more craven. If neurotic symptoms reduce fear, they will be learned. Other symptoms, such as ulcers, may be part of the direct physiological reaction to fear.

One way of reducing fear is to turn away from, suppress, or repress the fear-arousing stimuli. When mild fears are reduced in this way, the subject learns to avoid unpleasant topics; when intense fears are involved, complete repression may be reinforced. It is obvious that avoiding looking at and thinking about possible sources of danger may actually increase the probability of disaster or decrease the possibility of making the discrimination that no significant danger exists. Furthermore, anyone who wants to use fear as a motivation must effectively emphasize the proper escapes from fear or run the danger of having his subjects find their own escape by avoiding him and forgetting his message. In using fear as a drive it is important to remember that *the escape from fear is what reinforces learned behavior.*

Many additional practical points come out of the naturalistic study of fear, for example, the importance of knowing exactly what to expect or planning and knowing what to do to minimize the danger, of concentrating

on the task at hand, of breaking seemingly impossible tasks into manageable steps and concentrating on the successful performance of each step, the value of reassurance from the group or self-administered reassurance, the effectiveness of strong positive motivation and of rewards (counterconditioning) for nonfearful behavior. Additional discussion more oriented toward theory and experiment will be found in Miller [64].

*Curiosity.* In the course of a fruitless search for a learned drive based on hunger, Myers and Miller [91] applied to curiosity the same apparatus and theoretical paradigm that had been used in the study of fear. Curiosity had previously been brought into the laboratory from somewhat different points of view by the ingenious work of Berlyne [8], Harlow [32], and Montgomery [85]. In addition to applying the same operations used in the study of fear, Myers and I suggested the importance of determining whether a new drive, such as curiosity, shows the same pattern of deprivation-reinforcement-satiation as previously studied drives such as hunger. In other words, is there a correlation between the operations of reinforcement and satiation, so that an event which increases the subsequent performance of a response when administered immediately after that response is also found to tend to decrease the performance of responses motivated by that drive when it is administered in sufficient amount immediately before testing those responses? <sup>6</sup>

Finally, Kagan and Berkun [41] have used the same general paradigm for studying "activity drive" and Zimbardo and Miller [112] have used it as the point of departure for studying the effect of hunger on curiosity. *Drive elicited by perceptual mechanisms?* The general

<sup>6</sup> It should be noted that one can imagine special conditions under which the drive-stimulus-reduction hypothesis would not demand quite the same pattern as hunger seems to exhibit. Suppose pressing a bar turned off a shock which was almost immediately turned back on. There would be a correlation between the operations of reinforcement and satiation, but the period of "satiation" might be so brief that it easily could be missed. Furthermore, longer periods of "deprivation" might not produce greater amounts of recovery from "satiation."

approach can be further illustrated by another experiment. In an attempt to bridge the gap between gestalt theory, perceptual learning, and S-R theory, I wondered whether the vector toward a good gestalt had the properties of a drive. If it did, one might expect animals exposed to a bad gestalt to learn by trial and error a simple response that would cause it to change to a better one. The "bad gestalt" of a series of broken lines was projected in front of pigeons. Pecking on a response key caused this to change to the "good gestalt" of unbroken lines. For a control group, the same response caused a pattern of straight lines to change to one of broken lines.

To circumvent certain practical difficulties, we started with trained birds, tried to measure any reinforcing effect by a difference in resistance to extinction, and introduced a number of controls for possible stimulus generalization between the situations of training and of extinction.

The difference, although in the right direction, was relatively small and unreliable. Perhaps a tendency toward a good gestalt cannot function as a drive, or perhaps we used a technique that elicited it only weakly. But the experiment illustrates how the method can be applied.

With sufficiently ingenious apparatus, the same general method could be used to determine whether or not motivational effects are produced by sensory blurring, perceptual distortions, or the disruption of "phase sequences" that have been established by either perceptual experience or experimental training.

This paper has already suggested a way of applying the same general paradigm to the experimental study of motivation possibly produced by conflict.

*Motivation for achievement.* Observations of human and animal behavior suggest many additional possible sources of motivation for experimental study. For example, a child that has just learned to turn over from its stomach to its back may cry until it is placed on its stomach so that it can practice this fascinating act again. Can trial-and-error learning be motivated by this "drive"?

After the child has become skillful in turning over in

both directions, he seems to become bored with this activity but strives persistently against many difficulties for other skills—to pull himself up, to stand, to walk, and eventually to ski. What is the drive and reward for each of these skills? Is there a separate drive for every maturing potential skill or is there some mechanism to produce in infants what might be loosely labeled as a general drive toward achievement? If so, what kind of mechanism could possibly produce this result? How much of the adult motive is attributable to rewards that channel such a drive and how much to some drive that is elicited by social punishments for failure to improve and rewards for improving?

Conceivably, the blocking of almost any response tends to induce motivation and the partial or complete occurrence of the blocked response tends to relieve the motivation and function as a reward. This hypothesis might be checked by eliciting movements, such as circling, by central stimulation and determining whether such stimulation can motivate learning without any additional rewards, if, and only if, the movements are first blocked and then released. It is just barely conceivable that such a mechanism might be one step toward the development of a motivation that would have some of the functions of a drive to achieve.

*Certain ideas as motivation.* Similarly, observation of infants strongly suggests that they have something like definite ideas and that these can serve as transient but strong motivations. I have observed a child under the age of nine months seem to get the idea that he wanted to get a certain tray off a low shelf, struggle vigorously this way and that until he finally succeeded, and then show a look of joy and triumph.

In the adult it is obvious that, as the definite result of social training, certain thoughts, such as "I may have cancer," can elicit strong motivation. I have described such motives as *mediated learned drives* [64]. Could this mechanism have been involved in the infant, and if so, how, or was some more basic mechanism involved, and if so, what?

Perhaps, as I have suggested in a discussion of *goal-directed drives* [64], the discrepancy between an antici-

patory goal response (or certain other ideas) and the current state of affairs can be the basis for motivation. Again, we need to know far more empirical facts aimed at a penetrating analysis of the details of such behavior and its possible mechanisms.

*Possible response characteristics in infant hunger.* Let me conclude with one more concrete example. I have observed an infant during the first two weeks of life change within a few minutes from a state of relative quiescence to one of crying, reddening, and extreme activation so that my first reaction was to search for a jabbing pin. No such obvious source of activation was found, but when given a bottle, he drank vigorously and quieted down. If the milk in the bottle ran out, he became extremely activated, but by the time a new bottle could be heated, he sometimes had quieted down, was out of the mood, and refused it.

The suddenness of these shifts contrasts with what might be expected from the gradual accumulation (or restoration) of a physiological deficit; it suggests that hunger may have some of the properties of a response, being inhibited until it finally breaks through and then strengthens and maintains itself by positive feedback. On the other hand, perhaps it is not the hunger itself, but only the anticipatory goal responses or overt responses to the hunger, that have this characteristic.

In any event, it should be reasonably straightforward and profitable to devise objective techniques for recording the reactions of human or animal infants to hunger. Then one could systematically study the effects of a few simple controlled experimental operations of the type that frequently occur by chance in the natural environment.

*Additional problems.* In conclusion, let me emphasize that the foregoing examples are but a small sample of the types of naturalistic observations which point up the shortcomings of our current understanding of motivation. Each such observation could serve as the point of departure for a new line of research. I have made a number of suggestions, but these are meant more to point up the problems than to serve as definitive solutions to them. Many other problems remain to be raised. For example,

some motivations in some people seem to be relatively central in that they cannot be changed without a profound effect on the entire personality; others are peripheral and can be changed with little effect. What are the principles and conditions that cause different motivations to be central with different individuals? Why are some learned drives amazingly resistant to change, whereas others change easily? How are hierarchies of learned drives and rewards built up, and how do they interact?

### References

1. AULD, F., & MURRAY, E. J. Content-analysis studies of psychotherapy. *Psychol. Bull.*, 1955, 52:377-395.
2. BAILEY, C. J. The effectiveness of drives as cues. *J. comp. physiol. Psychol.*, 1955, 48:183-187.
3. BAILEY, C. J., & MILLER, N. E. The effect of sodium amytal on an approach-avoidance conflict in cats. *J. comp. psychol.*, 1952, 45:205-208.
4. BEACH, F. A., & HOLZ-TUCKER, A. M. Effects of different concentrations of androgen upon sexual behavior in castrated male rats. *J. comp. physiol. Psychol.*, 1949, 42:433-453.
5. BERGMANN, G., & SPENCE, K. W. Operationism and theory in psychology. *Psychol. Rev.*, 1941, 48: 1-14.
6. BERKUN, M. M. Factors in the recovery from approach-avoidance conflict. *J. exp. Psychol.*, 1957, 54:65-73.
7. BERKUN, M. M., KESSEN, M. L., & MILLER, N. E. Hunger-reducing effects of food by stomach fistula versus food by mouth measured by a consummatory response. *J. comp. physiol. Psychol.*, 1952, 45:550-554.
8. BERLYNE, D. E. Novelty and curiosity as determiners of exploratory behavior. *Brit. J. Psychol.*, 1950, 41:68-80.
9. BIRGE, JANE S. Verbal responses in transfer. Unpublished doctoral dissertation, Yale Univer., 1941.
10. BROWN, J. S. Factors determining conflict reactions in difficult discriminations. *J. exp. Psychol.*, 1942, 31:272-292.
11. BROWN, J. S. The generalization of approach responses as a function of stimulus intensity and strength of motivation. *J. comp. Psychol.*, 1942, 33:209-226.
12. BROWN, J. S. Gradients of approach and avoidance responses and their relation to level of motivation. *J. comp. physiol. Psychol.*, 1948, 41:450-465.
13. BROWN, J. S. Problems presented by the concept of ac-

- quired drives. In M. R. JONES (Ed.), *Current Theory and Research in Motivation: A Symposium*. Lincoln, Neb.: Univer. Nebraska Press, 1953.
14. BUGELSKI, R., & MILLER, N. E. A spatial gradient in the strength of avoidance responses. *J. exp. Psychol.*, 1938, 23:494-505.
  15. CAMPBELL, B. A. The fractional reduction in noxious stimulation required to produce "just noticeable" learning. *J. comp. physiol. Psychol.*, 1955, 48:141-148.
  16. CAMPBELL, B. A. Auditory and aversion thresholds of rats for bands of noise. *Science*, 1957, 125:596-597.
  17. CAMPBELL, B. A., & KRAELING, D. Response strength as a function of drive level and amount of drive reduction. *J. exp. Psychol.*, 1953, 45:97-101.
  18. CAMPBELL, B. A., & SHEFFIELD, F. D. Relation of random activity to food deprivation. *J. comp. physiol. Psychol.*, 1953, 46:320-322.
  19. CARNAP, R. Testability and meaning. *Phil. Sci.*, 1936, 3: 420-471.
  20. CHOY, M. Effects of unusual manipulations of "thirst" upon a variety of measures. Unpublished senior thesis, Yale Univer., 1956.
  21. CONANT, J. B. *On Understanding Science*. New Haven, Conn.: Yale Univer. Press, 1947.
  22. CONGER, J. J. The effects of alcohol on conflict behavior in the albino rat. *Quart. J. Stud. Alcohol*, 1951, 12:1-29.
  23. DELGADO, J. M. R., ROBERTS, W. W., & MILLER, N. E. Learning motivated by electrical stimulation of the brain. *Amer. J. Physiol.*, 1954, 179:587-593.
  24. DOLLARD, J. *Fear in Battle*. New Haven, Conn.: Yale Univer. Press, 1943.
  25. DOLLARD, J., & MILLER, N. E. *Personality and psychotherapy*. New York: McGraw-Hill, 1950.
  26. FREUD, S. *The Problem of Anxiety*. New York: Norton, 1936.
  27. FULLER, J. L. Personal Communication, 1956.
  28. GRINKER, R. R., & SPIEGEL, J. P. *Men Under Stress*. New York: McGraw-Hill-Blakiston, 1945.
  29. GRINKER, R. R., & SPIEGEL, J. P. *War Neurosis*. New York: McGraw-Hill-Blakiston, 1945.
  30. GROSE, ROBERT F. A comparison of vocal and subvocal conditioning of the galvanic skin response. Unpublished doctoral dissertation, Yale Univer., 1952.
  31. GUTHRIE, E. R. *The psychology of learning*. (Rev. ed.) New York: Harper, 1952.

32. HARLOW, H. F. Motivation as a factor in the acquisition of new responses. In M. R. Jones (Ed.), *Current Theory and Research in Motivation: A symposium*. Lincoln, Neb.: Univer. Nebraska Press, 1953.
33. HARLOW, H. F., MEYER, D., & SETTLAGE, P. H. The effects of large cortical lesions on the solution of oddity problems by monkeys. *J. comp. physiol. Psychol.*, 1951, 44:320-326.
34. HARRIMAN, A. E., & McLEOD, R. B. Discrimination thresholds of salt for normal and adrenalectomized rats. *Amer. J. Psychol.*, 1953, 66:465-471.
35. HEMPEL, C. G. The function of general laws in history. In H. Feigl & W. Sellars (Eds.), *Readings in Philosophical Analysis*. New York: Appleton-Century-Crofts, 1949. Pp. 459-471.
36. HULL, C. L. The goal gradient hypothesis and maze learning. *Psychol. Rev.*, 1932, 39:25-43.
37. HULL, C. L. The concept of the habit-family hierarchy and maze learning. *Psychol. Rev.*, 1934, 41:33-54, 134-152.
38. HULL, C. L. The goal gradient hypothesis applied to some "field-force" problems in the behavior of young children. *Psychol. Rev.*, 1938, 45:271-299.
39. HULL, C. L. *Principles of Behavior*. New York: Appleton-Century-Crofts, 1943.
40. HULL, C. L. *A Behavior System*. New Haven, Conn.: Yale Univer. Press, 1952.
41. KAGAN, J., & BERKUN, M. The reward value of running activity. *J. comp. physiol. Psychol.*, 1954, 47:108.
42. KAUFMAN, E. L., & MILLER, N. E. Effect of number of reinforcements on strength of approach in an approach-avoidance conflict. *J. comp. physiol. Psychol.*, 1949, 42:65-74.
43. KOHN, M. Satiation of hunger from food injected directly into the stomach versus food ingested by mouth. *J. comp. physiol. Psychol.*, 1951, 44:412-422.
44. LASHLEY, K. S. Learning: I. Nervous mechanisms in learning. In C. Murchison (Ed.), *The Foundations of Experimental Psychology*. Worcester, Mass.: Clark Univer. Press, 1929, Pp. 524-563.
45. LAWRENCE, D. H. Acquired distinctiveness of cues: I. Transfer between discriminations on the basis of familiarity with the stimulus. *J. exp. Psychol.*, 1949, 39:770-784.
46. LAWRENCE, D. H. Acquired distinctiveness of cues: II.

- Selective association in a constant stimulus situation. *J. exp. Psychol.*, 1950, 40:175-188.
47. LAWRENCE, D. H. The transfer of a discrimination along a continuum. *J. comp. physiol. Psychol.*, 1952, 45:511-516.
48. LEWIN, K. Environmental forces in child behavior and development. In C. Murchison (Ed.), *A Handbook of Child Psychology*. Worcester, Mass.: Clark Univer. Press, 1931.
49. LOGAN, F. A. A micromolar approach to behavior theory. *Psychol. Rev.*, 1956, 63:63-73.
50. MASSERMAN, J. H. *Principles of Dynamic Psychiatry*. Philadelphia: Saunders, 1946.
51. MEEHL, P. E. On the circularity of the law of effect. *Psychol. Bull.*, 1950, 47:52-75.
52. MILLER, N. E. The perception of children: a genetic study employing the critical choice delayed reaction. *Ped. Sem. J. genet. Psychol.*, 1934, 44:321-339.
53. MILLER, N. E. A reply to "sign-gestalt or conditioned reflex?" *Psychol. Rev.*, 1935, 42:280-292.
54. MILLER, N. E. The influence of past experience upon the transfer of subsequent training. Unpublished doctoral dissertation, Yale Univer., 1935.
55. MILLER, N. E. Analysis of the form of conflict reactions. *Psychol. Bull.*, 1937, 34:720.
56. MILLER, N. E. Reaction formation in rats: an experimental analog for a Freudian phenomenon. *Psychol. Bull.*, 1937, 34:724.
57. MILLER, N. E. Experiments relating Freudian displacement to generalization of conditioning. *Psychol. Bull.*, 1937, 36:516-517.
58. MILLER, N. E. An experimental investigation of acquired drives. *Psychol. Bull.*, 1941, 38:534-535.
59. MILLER, N. E. Experimental studies of conflict. In J. McV. Hunt (Ed.), *Personality and the Behavior Disorders*. New York: Ronald, 1944. Pp. 431-465.
60. MILLER, N. E. Studies of fear as an acquirable drive: I. Fear as motivation and fear-reduction as reinforcement in the learning of new responses. *J. exp. Psychol.*, 1948, 38:89-101.
61. MILLER, N. E. Theory and experiment relating psychoanalytic displacement to stimulus response generalization. *J. abnorm. soc. Psychol.*, 1948, 34:155-178.
62. MILLER, N. E. Social science and the art of advertising. *J. Marketing*, 1950, 14:580-584.

63. MILLER, N. E. Comments on theoretical models illustrated by the development of a theory of conflict behavior. *J. Pers.*, 1951, 20:82-100.
64. MILLER, N. E. Learnable drives and rewards. In S. S. Stevens (Ed.), *Handbook of Experimental Psychology*. New York: Wiley, 1951. Pp. 435-472.
65. MILLER, N. E. Fear. In R. H. Williams (Ed.), *Human Factors in Military Operations*. Chevy Chase, Md.: Johns Hopkins Univer., Operations Research Office, 1954. Pp. 269-281.
66. MILLER, N. E. The role of motivation in learning. Committee on Human Resources, Research and Development Board, Department of Defense. *Symposium on psychology of learning basic to military training problems*. Washington, D.C., GPO, May 7-8, 1953. Pp. 103-116.
67. MILLER, N. E. Shortcomings of food consumption as a measure of hunger: results from other behavioral techniques. *Ann. N.Y. Acad. Sci.*, 1955, 63:141-143.
68. MILLER, N. E. Effects of drugs on motivation: the value of using a variety of measures. *Ann. N.Y. Acad. Sci.*, 1956, 65:318-333.
69. MILLER, N. E. A psychologist speaks. In H. D. Kruse (Ed.), *Integrating the Approaches to Mental Disease*. New York: Paul B. Hoeber, 1958. Chap. 5, pp. 43-45.
70. MILLER, N. E. Learning and performance motivated by direct stimulation of the brain. In D. E. Sheer (Ed.), *Electrical Stimulation of the Brain: Subcortical Integrative Systems*. Houston, Tex.: Univer. Texas Press, 1957.
71. MILLER, N. E. Comments on the implications of the Olds reward effect for theories of reinforcement. In D. E. Sheer (Ed.), *Electrical Stimulation of the Brain: Subcortical Integrative Systems*. Houston, Tex.: Univer. Texas Press, 1957.
72. MILLER, N. E. Objective techniques for studying motivational effects of drugs on animals. In S. Garattini & V. Ghatti (Eds.), *Psychotropic Drugs*. Amsterdam: Elsevier, 1958. Pp. 83-103.
73. MILLER, N. E. Experiments on motivation: studies combining psychological, physiological, and pharmacological techniques. *Science*, 1957, 126:1271-1278.
74. MILLER, N. E., et al. *Graphic communication in the crisis in education*. Audio-Visual Communication Rev., Washington, D.C.: National Education Association, 1958, 5, No. 3.

75. MILLER, N. E., BROWN, J. S., & LEWIS, H. A theoretical and experimental analysis of conflict behavior: approach-avoidance conflict as a function of strength of drive and strength of shock. (Unpublished.)
76. MILLER, N. E., & DOLLARD, J. *Social Learning and Imitation*. New Haven, Conn.: Yale Univer. Press, 1941.
77. MILLER, N. E., & HART, G. Motivation and reward in learning. 15-min. Education Sound Film. University Park, Pa.: Psychological Cinema Register, 1948.
78. MILLER, N. E., & KESSEN, M. L. Reward effects of food via stomach fistula compared with those of food via mouth. *J. comp. physiol. Psychol.*, 1952, 45:555-564.
79. MILLER, N. E., & KRAELING, D. Displacement: greater generalization of approach than avoidance in a generalized approach-avoidance conflict. *J. exp. Psychol.*, 1952, 43:217-221.
80. MILLER, N. E., & MILES, W. R. Effect of caffeine on the running speed of hungry, satiated, and frustrated rats. *J. comp. Psychol.*, 1935, 20:397-412.
81. MILLER, N. E., & MILES, W. R. Alcohol and removal of reward: an analytical study of rodent maze behavior. *J. comp. Psychol.*, 1936, 21:179-204.
82. MILLER, N. E., & MURRAY, E. J. Displacement and conflict: learnable drive as a basis for the steeper gradient of avoidance than of approach. *J. exp. Psychol.*, 1952, 43:227-231.
83. MILLER, N. E., ROBERTS, W. W., & DELGADO, J. M. R. Motivation of learning by electrical stimulation in the thalamus. Motion picture presented at meetings of A.P.A. Cleveland, Ohio: Presidential Address of Experimental Division, Sept., 1953.
84. MILLER, N. E., & STEVENSON, S. S. Agitated behavior of rats during experimental extinction and a curve of spontaneous recovery. *J. comp. Psychol.*, 1936, 21:205-231.
85. MONTGOMERY, K. C. Exploratory behavior as a function of "similarity" of stimulus situations. *J. comp. physiol. Psychol.*, 1953, 46:129-133.
86. MOWRER, O. H. A stimulus-response analysis of anxiety and its role as a reinforcing agent. *Psychol. Rev.*, 1939, 46:553-566.
87. MURRAY, E. J. A content analysis method for studying psychotherapy. *Psychol. Monog.*, 1956, 70: No. 13, Whole No. 420. Pp. 1-31.
88. MURRAY, E. J., & BERKUN, M. M. Displacement as a function of conflict. *J. abnorm. soc. Psychol.*, 1955, 51:47-56.

89. MURRAY, E. J., & MILLER, N. E. Displacement: steeper gradient of generalization of avoidance than of approach with age of habit controlled. *J. exp. Psychol.*, 1952, 43:222-226.
90. MYERS, A. K. The effects of predictable vs. unpredictable punishment in the albino rat. Doctoral dissertation, Yale Univer., 1956. (In preparation for publication.)
91. MYERS, A. K., & MILLER, N. E. Failure to find a learned drive based on hunger; evidence for learning motivated by "exploration." *J. comp. physiol. Psychol.*, 1954, 47: 428-436.
92. OLDS, J., & MILNER, P. Positive reinforcement produced by electrical stimulation of septal area and other regions of rat brain. *J. comp. physiol. Psychol.*, 1954, 47: 419-427.
93. PAVLOV, I. P. *Conditioned Reflexes*. G. V. Anrep. (Trans.) London: Oxford Univer. Press, 1927.
94. PFAFFMAN, C., & BARE, J. K. Gustatory nerve discharges in normal and adrenalectomized rats. *J. comp. physiol. Psychol.*, 1950, 43:320-324.
95. PORTER, L. W., & MILLER, N. E. Training under two drives, alternately present, vs. training under a single drive. *J. exp. Psychol.*, 1957, 54:1-7.
96. RICHTER, C. P. Salt taste thresholds for normal and adrenalectomized rats. *Endocrinology*, 1939, 24:367-371.
97. SEARS, R. R., WHITING, J. W. M., NOWLIS, V., & SEARS, P. S. Some child rearing antecedents of aggression and dependency in young children. *Genet. Psychol. Monog.*, 1953, 47:135-234.
98. SCOTT, J. P. Social behavior, organization and leadership in a small flock of domestic sheep. *Comp. Psychol. Monogr.*, 1945, 18:(96)1-29.
99. SHEFFIELD, F. D., & CAMPBELL, B. A. The role of experience in the "spontaneous" activity of hungry rats. *J. comp. physiol. Psychol.*, 1954, 47:97-100.
100. SHEFFIELD, F. D., & ROBY, T. B. Reward value of a non-nutritive sweet taste. *J. comp. physiol. Psychol.*, 1950, 43:471-481.
101. SKINNER, B. F. Are theories of learning necessary? *Psychol. Rev.*, 1950, 57:193-216.
102. SOLOMON, R. L., SIDD, J. J., WATSON, P. D., & BLACK, A. H. The use of d-Tubocurarine in the extinction of fear in dogs. *Amer. Psychologist*, 1955, 10:395. (Abstract.)

103. SPENCE, K. W. *Behavior Theory and Conditioning*. New Haven, Conn.: Yale Univer. Press, 1956.
104. TINKLEPAUGH, O. L. An experimental study of representative factors in monkeys. *J. comp. Psychol.*, 1928, 8:197-236.
105. TOLMAN, E. C. *Purposive Behavior in Animals and Man*. New York: Appleton-Century-Crofts, 1932.
106. TOLMAN, E. C. There is more than one kind of learning. *Psychol. Rev.*, 1949, 56:144-155.
107. VON FELSINGER, J. M. The effect of induced estrus as an irrelevant drive on the learning of a maze habit and on its persistence during satiation. Unpublished doctoral dissertation, Yale Univer., 1948.
108. WEBB, W. B. The motivational aspects of an irrelevant drive in the behavior of the white rat. *J. exp. Psychol.*, 1949, 39:1-14.
109. WHITING, J. W. M., & CHILD, I. L. *Child Training and Personality*. New Haven, Conn.: Yale Univer. Press, 1953.
110. WICKERT, F. *Psychological research on problems of redistribution*. Washington, D.C.: GPO, 1947.
111. YOSHIOKA, J. G. Weber's law in the discrimination of maze distance by the white rat. *Univer. Calif. Publ. Psychol.*, 1929, 4:155-184.
112. ZIMBARDO, P. G., & MILLER, N. E. The facilitation of exploration by hunger in rats. *J. comp. physiol. Psychol.*, 1958, 51:43-46.

## Index

- Activity level, 162, 199, 202
- Afferent impulse, 2
- Androgen level, 182
- Achievement, 203-204
- Anxiety, 44, 91, 93-94
- Belief-value, 62, 65, 77
- Behavior space, 69
- Boredom, 191
- Brain stimulation, 188, 190
- Conditioning, classical, 91
- Contiguity hypothesis, 191
- Cue, 46
- Curiosity, 199, 202
- Deprivation interval, 107
- Displacement reaction, 131
- Drive, as an acquired response, 81
  - biogenic, 105
  - disanimation, 17, 150
  - effect on threshold, 182
  - energizing, 82, 151, 154, 172
  - generalization, 180
  - goal-directed, 204
  - innate, 105
  - learnable, 194
  - mediated learned, 204
  - non-homeostatic, 118
  - primary, D, 23, 149
  - psychogenic, 105
  - reduction hypothesis, 185
    - test of, 187
  - regulation, 187
  - reinforcement and, 189
  - secondary, 43, 86
  - self-actualization, 140
  - sex, 196
  - stimulus, 19, 38, 42, 87-88, 149-155, 180, 183
    - to perceive and know, 170, 203
    - unit (mote), 23
- Drug, effects on extinction, 20-22
- Ego-involvement, 145
- Ethos, 67
- Extinction, 4, 8-10, 53
- Fear, 199-201
- Functional similarity, 63
- Generalization, drive, 180
  - effect of drive on, 171
  - primary stimulus, 19, 25, 29
  - stimulus, 180
- Goldstein, K., 140
- Guthrie, E. R., 191
- Habit, strength of  $sH_n$ , 22, 149, 165
- Hull, C. L., 1, 139, 148, 190
- Imprinting, 196-197
- Instinct, 112
- Law of Effect, 3-4
- Learning, and channeling, 194
  - and elucidation, 194
  - at different deprivations, 162
  - conditioned reflex, 2
  - dilemma, 58
  - latent, 189
  - simple selective, 2
- Mote, 23
- Motivation, achievement, 203
  - capacity as, 120, 139

- initiation of, 7
- secondary (incentives), 7
- Mowrer, O. H., 199
- Need, 7
  - achievement, 73-74
  - affiliation, 73-74
  - organic, 114, 141
  - primary theory, 139
  - system, 69-70
- Pavlov, 2, 34, 35, 53
- Patterning, 36
- Perception, 203
- Perceptual sensitization, 89
- Reaction-evocation potentiality,  $sE_R$ , 23, 32, 149
- unit (wat), 23
- Reinforcement, 1, 53
  - increase in pleasure, 186
  - need-onset hypotheses, 5
  - primary, 1, 142
    - law of, 2, 4
  - secondary reward, 54
  - threshold, 188
- Response, 48
  - initial hierarchy, 49
  - innate hierarchy, 49, 190
  - produced drives, and rewards, 182
  - resultant hierarchy, 49
  - threshold, 107
  - unit acts, 115
- Reward, 54 (see Reinforcement)
- Sensitization, 107
- Sexual, activity in rats, 13
  - learning in chimpanzees, 133
- Spence, K. W., 148, 152, 190, 193
- Stimulus, 125
- Stimulus gradient, 181
- Task-involvement, 145
- Tolman, E. C., 193
- Thorndike, E. L., 3
- Trace, 2
- Two-factor theory, 192
- Watson, J. B., 23
- Weber's Law, 188



Form No. 3.

PSY, RES.L-1

**Bureau of Educational & Psychological  
Research Library.**

The book is to be returned within  
the date stamped last.

.....	.....	.....
.....	.....	.....
.....	.....	.....
.....	.....	.....
.....	.....	.....
.....	.....	.....
.....	.....	.....
.....	.....	.....
.....	.....	.....
.....	.....	.....
.....	.....	.....
.....	.....	.....
.....	.....	.....
.....	.....	.....